

J. H. Paine
49

AN

EXPERIMENTAL
ENQUIRY



CONCERNING

THE CAUSES WHICH HAVE GENERALLY
BEEN SAID TO PRODUCE PUTRID
DISEASES.

By WILLIAM ALEXANDER, M. D.

Τινες αιτιαι—

— Διαφέρει γαρ πολυ.—

ARISTOT. ARS POET.

LONDON:

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MDCCLXXI.



TO
SIR JOHN PRINGLE, BART.
AND
PHYSICIAN TO HER MAJESTY,
THE FOLLOWING ENQUIRY
IS MOST RESPECTFULLY DEDICATED,

BY
HIS MOST OBEDIENT
HUMBLE SERVANT,

Wm. ALEXANDER.

TO
SIR JOHN PRINGLE, Bart.
AND
PHYSICIAN TO HER MAJESTY
THE FOLLOWING ENQUIRY
IS MOST RESPECTFULLY DEDICATED.



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CONCERNING

The CAUSES which have been
generally said to produce
PUTRID DISEASES.

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INTRODUCTION.

WHEN we take a view of the various and numerous diseases which from the earliest ages of the world have afflicted and destroyed the human race, we find that these of the putrid kind, whether endemic or epidemic, have always been the most dreadful, and made the greatest carnage: while others have, in a long revolution of years, thinned families, these have at one blow depopulated

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cities

cities and kingdoms: while others have here and there marked out single victims, these have indiscriminately mowed down whole armies, and laid the victors and the vanquished together peaceably in the dust.

From considering putrid diseases in this light, as the most dreadful, the most destructive of all others; an enquiry into their causes, into their nature, and the methods of preventing them, will appear the more important, and deserve our more serious attention; not because such enquiries have not frequently been made, but because they have always, so far as I know, been conducted upon theoretical reasoning, and not upon experiments and facts, which are the only sure foundations of knowledge concerning the operations of nature.

All dead bodies naturally run into a putrid state, unless preserved from it by some antiseptic; but had putrefaction never attacked animals till after death, its nature would not have become so much an object of universal enquiry.



enquiry. As it appears, however, frequently in living animals, where it gives rise to one of the most dreadful and fatal diseases that ever afflicted the human race, this naturally rouses the attention, and produces a general desire to investigate its nature and causes; and accordingly a variety of ingenious hypotheses have from time to time been introduced to account for it. Earth, sea, and air, and a variety of other causes, have been considered as productive of it. I will not deny but some of these, at particular times, may be the cause of putrid diseases; but at the same time suspect the cause will be more commonly found existing in our own bodies, as a consequence of the follies and irregularities of our lives.

There is a kind of uniform simplicity observed by Nature in performing all her operations. Those employed in the science of physics seem to have kept this constantly in their view, and to have made so much use of it, that whoever has been able to

give the most simple account of the cause of any natural phenomenon, has generally been supposed to have given the most elegant and agreeable one.

Led by this principle, the philosophers and physiologists of former ages, observing that animal substances putrefied soonest when kept in a warm moist place, from thence concluded that the sole cause of putrefaction was certain degrees of heat and moisture which were not yet fully ascertained, and various in various substances. Others, something more complex in their ideas, have added to this account the putrid effluvia arising either from animal or vegetable substances, while resolving into their first principles.

There have been some also who, not satisfied with any of these methods of accounting for putrefaction, and observing that a great number of animalcula are discovered in almost all putrid substances, have from thence concluded that the ova of these animalcula,

malcula, being every where disseminated, and hatched upon such bodies as afforded them a proper nidus, by gnawing and destroying the texture of these bodies, were the sole cause of their running into a putrid state.

A late ingenious author* supposes he has discovered a more natural and easy solution of the modus of putrefaction, by considering it as arising from the loss of a cementing principle, by which the various parts of any body adhere together, and by which adhesion, he imagines, they are preserved in their natural state, and defended from putrefaction.

As sailors in long voyages have frequently been attacked with that species of putrid distemper called the sea scurvy, the salted, and perhaps damaged and mouldy provisions upon which they are obliged to subsist, have likewise been much blamed as the causes of putrefaction. And so much have philosophers in all ages been

* Dr. Mac Bride.

puzzled in accounting for putrid malignant disorders, that several have deduced them from earthquakes, the poison emitted from serpents, and other causes still more ridiculous*.

As we are constantly surrounded with the air, as it is constantly entering into our lungs, has such a variety of particles floating about in it, and is subject to so many sudden mutations, many have supposed that, from the nature of those particles and mutations, putrid diseases have frequently had their origin †.

I shall examine these causes separately, with a view to discover by experiments, whether any one of them is of itself able to produce any of those diseases that are generally denominated putrid and malignant, or whether such diseases do not rather commonly depend on a concurrence of

* Kircher de caus. pest.

† Plin. Hist. Nat.

several of them*, and perhaps on some others also which have not hitherto attracted so much the attention of mankind, nor been so much the subject of their enquiry.

* By putrid malignant distempers, as often as they occur through this work, the author means such as are attended with black, livid, or purple spots; spongy bleeding gums; bloody foetid stools; or any of the other symptoms which generally indicate a dissolved state of the blood and juices.

C H A P. II.

*Of Heat, whether it is necessary in order to
bring on Putrefaction.*

H EAT may not improperly be considered as an immense chain, with both the extremities of which we are as yet, and perhaps ever shall be, entirely unacquainted; and as we can neither discover the greatest degree of possible heat, nor of possible cold*, we must consider ourselves as having only some imperfect knowledge of a few of the intermediate links of this great chain; and even of this intermediate part which falls within the compass of our knowledge, there are but a few points or degrees, which seem to favour the process of putrefaction; and it is certain that there are two points, both within this

* Cold, according to many philosophers, is only reckoned a privation of heat, and not produced by the positive agency of frigorific particles.

space,

space, at or beyond either of which there is an absolute stop put to it; those are the freezing point, and that other, at whatever part of the scale it may exist, where the moisture necessary to putrefaction is exhaled so fast as to prevent any substance from having time to run into that particular state.

It is a fact, long established by experience, that all substances, whether animal or vegetable, remain fresh while they continue in a frozen state; for the northern nations, according to the accounts given us by the histories of Greenland, and of several other places near the arctic circle, preserve their winter provisions by burying them in the snow, as effectually as we do by the means of salt. It is no less certain that there are degrees of heat, which, in direct opposition to this method, preserves the same substances, by exhaling their radical moisture so fast that they have not time to run into the putrid ferment: of this, Nature is pregnant every where with examples;

examples ; but I shall only mention the great quantities of fish that are every year cured without salt upon the banks of Newfoundland, and other places where fisheries are established. It is between the freezing point therefore, and that which is necessary to exhale this moisture before a fermentation can come on, that we must enquire for the degree of heat requisite to bring on putrefaction.

The freezing point has long ago been exactly determined, but hitherto we do not know that point or degree of heat at which the moisture of bodies begins to exhale, far less do we know that degree at which it exhales so fast as to hinder the putrefactive ferment from taking place. This, indeed, may perhaps be relatively, but it never can be positively determined; for a piece of meat cut very thin will soon be deprived of its natural moisture by any of the degrees of heat between 70 and 80 ; whereas a whole joint will only be a little dried upon the

the surface, and generally become putrid in 48 hours, in any of these degrees of heat by which the other was intirely hindered from becoming putrid. But in order to convey a more distinct idea of this matter, I shall lay before the reader some experiments made concerning it.

EXPERIMENT I.

A piece of beef, of about a quarter of an inch thick, was laid on a china plate, and set in a closet where Fahrenheit's thermometer stood at 72 degrees; in sixteen hours it became almost perfectly dry, and had not acquired any perceptibly putrid smell.

Another piece, of about half an inch thick, became nearly as dry as the former in about 38 hours, and had acquired some degree of fætor, though little more than just enough to be perceptible.

A third piece, of about an inch thick, became nearly as dry as the first
in

in four days, and had got a smell something more foetid than the last.

A fourth piece, two inches thick, in two days smelled pretty much; the third day it smelled so exceedingly ill that I was obliged to throw it away. During all this time the mercury in the thermometer was never beneath 70, nor above 74.

The same experiments were tried in a cooler place, where the mercury in the thermometer commonly stood between 64 and 68. Here the piece of a quarter of an inch thick took almost two days to become as dry as a piece of the same thickness had done in 16 hours at the heat of 72 degrees: it had likewise acquired a perceptibly putrid smell.

A piece of half an inch thick, in this degree of heat, seemed only to have suffered some little exsiccation on its surface in the space of three days; on cutting into its internal parts, it had lost its colour, and smelled disagreeably.

A third

A third piece, of an inch thick, at the end of two days had become rather more putrid than the piece of half an inch thick had done in three days. The mercury in the thermometer, greatest part of the time that this piece was upon trial, had stood at 67 and 68. From what had happened to these pieces, I thought it would be needless to try what would happen to a piece of two inches thick in this degree of heat, and therefore proceeded to try what would happen in a superior degree.

A piece of beef, of a quarter of an inch thick, being laid in the same manner on a plate in a closet where the mercury generally stood from 86 to 90 degrees*, was become almost dry in about 12 hours, and had no disagreeable smell.

* This closet, in which several of the subsequent experiments were made, was divided by a stone wall from the kitchen fire; the heat of which fire, which was never suffered to go out at night, generally kept the closet of the temperature I have mentioned above.

Another piece, of half an inch thick, was become nearly as dry as this last in about 28 hours, and had not got any ill smell that I could discover.

A third piece, of an inch thick, was become not quite so dry as the last two in three days, and had just a perceptible degree of ill smell.

A fourth piece, of two inches thick, could not be dried fast enough by this degree of heat, to hinder it from becoming putrid, which it did in one night.

The whole of these trials taken together plainly point out to us, that the degree of heat necessary to dry bodies so fast as to hinder them from running into the putrid ferment, depends intirely upon the thickness, and quantity of moisture of these bodies. But as this subject is rather a matter of curiosity than of utility, I shall proceed to some experiments, instituted with a view to discover what degree
of

of heat is most favourable to putrefaction.

Boerhaave has assigned the degree of 70 as the properest for generating putrefaction in animal substances; and Shebbeare, the only other author I know of who has mentioned this subject, says, that by repeated experiments he had found a heat from 70 to 75 and 80, the true heat for producing putrefaction.

EXPERIMENT II.

I filled two four-ounce phials up to their narrow necks with human blood, fresh drawn from a pretty healthful person; one of these phials I set in a heat which varied between 70 and 74; the other I set in the above mentioned closet, where the heat varied between 86 and 90. The blood in the first phial did not begin to emit any putrid smell till it had stood about 52 hours. That in the second smelled perceptibly foetid in 36 hours,

As

As the event of this experiment was so different from what is mentioned by Boerhaave, I repeated it again with fresh drawn bullocks blood, not being able, just at that time, to procure any human blood. The blood in the phial which stood in a heat between 70 and 74 was become perceptibly foetid in 48 hours; that in the phial which stood in a heat between 86 and 90 was perceptibly foetid in 33. Hence it would seem that bullocks blood becomes sooner putrid than human blood, which may happen because bullocks blood abounds more with crassamentum, which is the most putrescible part of that fluid.

EXPERIMENT III.

I next resolved to try what would happen to solid animal substances in the same degrees of heat in which the blood had been tried. Accordingly, two pieces of beef, each two inches thick, were laid upon two saucers, and placed the one in the heat that varied
between

between 70 and 74, and the other in the heat that varied between 86 and 90. The piece in the lowest degree of heat smelled very perceptibly foetid in 46 hours. The other in the greater degree of heat smelled foetid in about 23 hours. This last piece being kept several days in the same place, at last became almost perfectly dry; when I observed that the foetor which it had once acquired was much diminished, by which it would appear that its most fluid parts, which were now dissipated by the heat, were the parts that had been most affected by the putrefaction.

EXPERIMENT IV.

As I had now satisfied myself that both animal fluids and solids became sooner putrid in a degree of heat far above that assigned as the properest for bringing on putrefaction by Boerhaave, than they did in it, I now resolved to see whether still greater degrees of heat would not still more accelerate the putrefactive process in
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human

human blood; and for this purpose put four ounces of it fresh drawn into a phial with a narrow neck as before, to prevent the evaporation that would have happened by exposing a larger surface to the air. This phial I placed in a sand heat of 100 degrees; the blood in it soon after began a sort of intestine or fermentative motion, never separated into crassamentum and serum, and began to emit a foetid smell in about 18 hours.

Another phial, containing the same quantity of fresh drawn blood, was placed in the same sand heat, and the degree of heat raised to 110. The appearances in this blood were nearly the same as in the last, and the foetid smell began to arise from it in about 17 hours. A piece of beef, weighing $\bar{3}$ iv. was laid at the same time upon the surface of the sand; in about 24 hours it was become almost as hard as a piece of horn, and had no putrid smell. Another piece of the same weight, placed at the same time on the sand heat in a bowl of water, began to have

have a putrid smell rather sooner than the blood.

This experiment is a proof of what I took notice of before, viz. that the degree of heat which will soonest of all others bring on putrefaction, cannot be positively determined, but must vary according to the magnitude, moisture, and qualities of the substance made trial of.

EXPERIMENT V.

The sand heat being raised to 130 degrees, another phial containing four ounces of fresh drawn blood was placed in it. The appearances in this phial were different from what they had been in the last; for in this a kind of intestine motion soon began, and continued always nearly the same. The crassamentum and serum sometimes seemed to have separated imperfectly from each other, and again to have united into one mass; sometimes a sort of coagulum seemed to be forming on the surface of the

C 2

blood,

blood, and then it would disappear again. After about 16 hours a smell, different from that of fresh blood, began to arise from it; but it was not perfectly that putrid smell that had arose from the others, but something between a volatile, urinous, and putrid smell.

A piece of beef weighing 3iv. which had been placed in a bowl of water on the sand heat at the same time, instead of becoming putrid, turned hard, and contracted into less than its original magnitude, and scarcely gave any bloody tinge to the water in which it was infused.

Another phial of blood being tried in 140 degrees of the same sand heat, exhibited nearly the same appearances as the last, and had also a volatile disagreeable smell, nearly of the same kind.

A bowl of water placed at the same time in this degree of heat, with 3iv. of beef in it, exhibited also nearly the same appearances as the last, only the beef was a little harder, and the water

ter had still less of the bloody colour.

From all these experiments taken together, it appears that the degree of heat which soonest brings putrefaction upon animal substances is much above 70. But here we only mean dead animal substances; for we have hitherto made little or no progress in discovering that degree which proves most favourable to the putrefaction of living ones. Boerhaave, in his attempt to discover this by shutting a dog, a cat, and a sparrow in an oven, has made it much greater than that which he thought would soonest bring it upon dead animals; but the conclusions he has drawn from this experiment appear from later discoveries to have been founded upon false principles.

Although it appears from what has been already said, that putrefaction cannot possibly happen but at one of these degrees of heat that are between the freezing and the boiling points of Fahrenheit's scale; and although, in

solids, it will probably happen soonest between 90 and 100, and in fluids between 100 and 110 of that scale, yet none of these degrees necessarily bring it on; and a living animal may be kept in any one of them that it is able to bear, and part of a dead one in any one of them, without having any tendency to putrefaction, provided none of the other causes of putrefaction besides heat are present. Therefore though it appears from a variety of facts, that certain degrees of heat are absolutely necessary toward inducing and carrying on the process of putrefaction. It seems plain also, that no degree of heat can be the sole cause of it.

C H A P. III.

Of Moisture; whether it is absolutely necessary to Putrefaction?

THAT moisture is a necessary agent in the process of putrefaction, is a fact that has been so long established, and appears so obvious, that it stands in need of no experiments to confirm it. The doctrine of heat and moisture being productive of putrid diseases*, is as old as Hippocrates, and has been corroborated almost by every subsequent author who has treated of the subject. The drying of fish in the sun, which I mentioned before, and a variety of other natural operations every day going on before us, put the matter beyond all doubt.

In what proportion the fluid parts of any body must be to its solid parts, in order to fit it for becoming putrid,

* Hippocrat. Epidemic.

has not hitherto been attempted to be determined; nor indeed does it seem a matter of any great consequence, either with regard to the prevention or cure of putrid diseases. It has been thought that some degree of fermentation is always necessary toward producing putrefaction. Whether it is really so, I cannot determine; but it is certain, that several substances, containing a proportion of fluid particles by much too small to allow of any visible fermentation, do, without any such fermentation that we can discover, though by a process perhaps not truly putrefactive, lose their uniting principle, whatever it is, and crumble as it were spontaneously into dust. This every one must be convinced of, who has seen rotten wood taken out of old buildings or other places, where all the ingenuity of art has been employed in order to secure it from moisture.

This spontaneous kind of powder, so far as I have hitherto observed, seems not to have been the work of a ferment

ferment and consequent putrefaction, but of an amazing number of animalcula, which prey upon its substance, destroy the texture of its parts, and so reduce it into a kind of dust.

This seemingly spontaneous resolution of dry bodies, though it has effects pretty similar to these of putrefaction, appears nevertheless to be widely different from it, being generally attended with little or almost no smell, nor possessing, as far as I can discover, any power of accelerating putrefaction in other bodies to which it is applied, as will appear from the following experiments.

EXPERIMENT VI.

Three pieces of fresh beef were laid upon three china saucers. One was covered with dust of rotten wood, and the other with dust of mouldy barley, both of which were crowded with animalcula; the third was covered with saw-dust of fresh wood, in which no animalcula could be discovered.

They

They were all placed together in a warm closet, and as near as I could observe, began all to turn putrid about the same time. Now had the dust of rotten wood, or of the mouldy barley, had any power of corrupting animal substances, the pieces of beef covered with them should have become sooner putrid than that piece in the fresh saw-dust. But as this did not happen, it is doubtful whether they have any power of accelerating putrefaction in animal bodies.

EXPERIMENT VII.

Having tried whether this vegetable matter that had suffered a spontaneous resolution, would communicate putrefaction to animal substances, and found it did not, I resolved next to try whether it would affect vegetable substances. Accordingly having got three small turnips, all taken out of the ground at the same time, I laid them upon three saucers, covered them as in the last experiment, and
set

set them in a damp warm place, but could not discover the symptoms of putrefaction any sooner in these that were covered with the dust of rotten wood and of barley, than in that which was covered with the fresh saw-dust; and hence I concluded that this resolution of dry bodies is a resolution *sui generis*, someway differing from the true putrefactive resolution, which does not appear to happen without as much moisture as is necessary for fermentation.

But though moisture, or in other words, some degree of fluidity, is absolutely necessary in order to fit either animal or vegetable substances for running into the putrid ferment, yet we know not what kind of moisture gives the greatest proclivity for this ferment. Water, in its pure elementary state, is, perhaps, not susceptible of putrefaction, as appears by distilled waters keeping very long sweet and transparent; and, perhaps, could they be intirely freed from any heterogeneous bodies, they would keep so rever.

But

But though water in its pure elementary state be, perhaps, not susceptible of putrefaction, yet when certain proportions of it are added to other substances, it greatly accelerates their running into that state, and in other proportions hinders their running into it. Thus, wood upon which water is frequently sprinkled will soon rot, whereas wood always kept under water will remain fresh a great number of years. But as it is of no great consequence to know what sort of moisture it is that soonest disposes animal bodies to become putrid, I shall proceed to consider the proportion of moisture that soonest disposes them to become so.

It is almost impossible to determine the exact quantity of solids and fluids in the composition of any body ; in order however to attempt to discover what quantity of fluids was necessary in order to keep a piece of meat in a state fit for becoming putrid, I took four ounces of beef, and having by heat evaporated an ounce of its moisture,

ture, I wrapped it in a piece of oiled ox-bladder, to prevent any further evaporation: It became putrid in about 50 hours. Another piece of beef of the same weight, was dried till it had lost an ounce and a half, and being wrapped in a bladder as before, it began to turn putrid in about 60 hours: Another, being dried till it had lost about an ounce of its weight, hardly ever acquired any putrid smell. This evaporation to one half was almost the greatest length I could go, without making the beef almost as hard as wood.

Four ounces of fresh beef was put into a strong rag, and squeezed pretty hard in a screw-press; when taken out, it weighed $\text{ziii. 5i. gr. iii.}$ and being wrapped in a bladder it became putrid in about two days.

From this it would seem that, in proportion to the quantity of moisture contained in any animal or vegetable body, so much the sooner does that body putrefy. At least this seems to take place in such bodies as
are

are called solids, which is illustrated by the common operation of smoking hams, tongues, &c. which, I have been told, will not keep near so long unless they have acquired a certain degree of dryness. But for discovering this degree the dealers in those commodities have as yet no certain rule: perhaps a rule might be discovered by trying their original weight, and how much of that weight they have lost when they are in the most proper state for keeping. It is further illustrated by the preparation of vegetables for forming a *hortus succus*, which will not keep unless they be thoroughly dried.

Since it appears that a part of a dead animal putrefies the sooner the more moisture it contains, it would be curious to observe, whether a redundancy of moisture has the same effect upon living animals; and whether lusty corpulent people, who are seemingly full of juices, are more liable to be attacked with putrid diseases than thin meagre ones. I have,
for

for some years past, paid a little attention to this matter, but cannot as yet determine any thing concerning it. Though it would seem to me, that from the quantity of the juices of any animal we can hardly form any idea of its greater or less proclivity towards putrefaction; as every living animal, even in its most exhausted state, must still be possessed of as much as is sufficient for fermentation, which is all that is necessary towards keeping it fit for putrefaction: we must rather judge from the quality of those juices, so far as we can discover their qualities; and, in forming this judgment, the more crude, watery, and indigested, and the less animalized these juices are, it will, *ceteris paribus*, be presumable to suppose the animal the more liable to putrid diseases; and this coincides with the observations of several of the best practical authors, who have generally agreed that such people as were debilitated either by former diseases, low poor living, &c. were the most subject to putrid diseases,

eases, and the soonest overcome by them.

From the whole of these observations we may conclude, that moisture is, in some degree, absolutely necessary to putrefaction both in animals and vegetables ; but from the method of preserving dead animals and vegetables by exsiccation ; no inference can be drawn of any utility to the living ones, as no attempt can be made to preserve them in that manner.

C H A P. IV.

Of the Effects of putrid Effluvia on the Process of Putrefaction.

EFFLUVIA may not improperly be divided into four different kinds, viz. the animal, vegetable, mineral, and mixed, which comprehend all these that can possibly affect our atmosphere.

Animal effluvia may again be subdivided into that kind which arises from a number of dead bodies of any sort putrefying in the open air, as happens sometimes on a field of battle where there has been a great slaughter; or from the accidental destruction of locusts, or other animals which multiply in prodigious numbers; that which arises from the bodies of diseased animals by perspiration, sweat, breath, &c. and that which arises from their excrements, whether they be diseased or in health.

That the effluvia arising from a field of battle where there has been great

D

slaughter,

slaughter, or from a number of any other animals putrefying together in a hot country is contagious, although some have denied it, seems nevertheless to have been confirmed by the experience of all ages*. Dr. Lind observes, that, at Bencoolen, the diseases that always rage violently during the month of October, are occasioned by dead fish and other animals left by the Ganges; and that the unhealthfulness of Gambroon arises from vast quantities of little fishes left upon the shore, which soon become highly putrid and contaminate the air †. Livy mentions a plague that arose from the effluvia of dead carcases putrefying on a field of battle ‡. And Kircher takes notice of several instances of the same kind having arisen from locusts, and other animals suddenly destroyed, and lying till they became putrid in the open air §.

* Lind's Observations.

† Lind's Essays on the Diseases incident to Europeans in hot Climates.

‡ Liv. Hist. Rom.

§ Kircher de caus. et effect. pestis. & Ludolf. Histor. Æthiop.

Although

Although it is a possible case, that this kind of animal effluvia may sometimes produce putrid epidemic diseases, yet if we seriously examine the matter, they will perhaps be found to happen much seldomer in this manner than is generally supposed, and then too, only in the warmer and less ventilated parts of the world: For it seems evident from the nature of a mountainous, and consequently well ventilated country, that hardly any quantity even of the most putrid effluvia, can hurt the health of the inhabitants, as it never can hang long enough in the air to be exalted into a proper degree of virulence, but is generally soon dispersed by some sudden gust of wind*.

This observation seems exactly to quadrate with the experience of all the northern nations, and more particularly with that of Britain, where we destroy more animals to feed our luxury than any other nation, and yet

* But no kind of putrefaction is ever heightened in these European countries, to a degree capable of producing the true plague. *Mead on Pest.*

never find any putrid epidemic taking its origin from the steams arising from their blood, intestines, and other offal; though were it to lie exposed near the torrid zone in the same manner as it does with us, it would perhaps prove the source of numberless calamities.

But though the northern countries, both on account of their coldness and of the violent winds which often sweep so impetuously over them, be pretty much exempted from the effects of contagious effluvia arising from putrid animal matter, we are not from thence to infer that no part of the world can suffer by this effluvia. For we who inhabit the colder and more sterile climes, can hardly form any adequate idea of the amazing fecundity of earth, sea, and air, in some of the warmer ones, where reptiles, insects, and a variety of other animals, multiply, die, and putrefy in such surprizing numbers.

When, therefore, these amazing multiplications happen, as is frequently the case, to be accidentally
I
destroyed

destroyed in a warm climate, almost wholly overgrown with wood, and extended for many leagues into a level, where an uninterrupted calm almost perpetually reigns, the effluvia arising from such carnage and destruction remains undisturbed in the atmosphere till it is exalted by the heat of the sun into a proper degree of virulency for contaminating the surrounding objects, from one of which it is propagated to another, till at last it spreads to a considerable distance; and hence the insalubrity of the air in the proximity of putrid animal matter in hot countries appears to arise*.

Though we have no instance of any quantity of putrid matter ever having contaminated our northern atmosphere, yet we are too well assured that it has often contaminated particular bodies, which have either come into actual contact with, or approached too near it. We are likewise convinced, by many experiments, that a

* Vid. J. Léo. Hist. Afric.

piece of meat suspended over a piece of putrid animal flesh will sooner spoil than another that is suspended in the open air. On these accounts, therefore, though we have no great reason to dread a putrid contagion being universally diffeminated through the atmosphere of our country, yet we ought cautiously to beware of particular ones, which have often proved destructive, not only to inattention and temerity, but sometimes also to the greatest caution and prudence.

But leaving this subject, let us proceed to the perspirable matter, which, that it has a septic power, will appear from the following experiments.

EXPERIMENT VIII.

Two pieces of beef of an equal weight were wrapped, one of them in a piece of linen rag thoroughly moistened with the sweat of a healthy person, the other in a piece of rag wet with pure water: They were then both laid in the above-mentioned closet.

closet. The piece that was wrapped in the sweaty rag began to putrefy three or four hours sooner than the other.

I have not hitherto had convenience for making any trial of this kind with the sweat of a diseased person; but if the sweat of one in good health have a septic power, as seems to appear from this experiment, that of a person even in the slightest distemper will have, perhaps, a still greater one. What must we then expect from the sweat of one already labouring under a disease, where the whole mass of humours have run into a dissolved and putrid state?

That the breath of healthful people has a considerable septic power I have already endeavoured to prove in another place*, and therefore shall not repeat any thing concerning it here; but only remark, that since even the breath of healthful people has a septic power, what have we not to fear

* Experimental Essays.

from that of such as are afflicted with putrid diseases. I would therefore caution all people against exposing themselves to the steam or breath of a diseased animal body, and particularly if the disease be of the putrid kind. Against shutting themselves too closely up in a room with such an one, for in this case the whole air of the room is soon filled with the morbid particles continually flying off from that body, by which any person in it runs the greatest risk of being attacked with the disease of the patient he is attending.

Several, both ancient and modern writers, in enumerating the various causes of the putrid dysentery and fever, which so frequently attacks armies that are encamped in rainy seasons, and towards the beginning of winter, have reckoned the effluvia arising from the privies as one of the most active and virulent.

Doctrines that have been handed down to us from our ancestors through a long succession of generations,

tions, deserve at least a serious consideration, and ought not to be exploded till we are convinced of their falsity by observations and experiments. The following experiments therefore were tried with a view to discover whether the fœcal matter had any septic or contagious power.

EXPERIMENT IX.

A piece of fresh mutton was suspended in the steam arising from a necessary-house, and another piece of the same weight hung over a basin of pure water, which stood on the floor of the same necessary-house. The piece suspended in the steam of the necessary-house remained sweet two days after the piece over the pure water had begun to putrefy. It was then lost by accident.

EXPERIMENT X.

A fresh raddish was let down by a pack-thread into the same fœcal matter, and remained there all night; on
being

being taken up in the morning it was changed into a dark green colour.

From these experiments, as well as from the smell of putrid excrement, it seems pretty evident that it contains a considerable quantity of volatile alkali, which the learned Sir John Pringle has in several of his interesting experiments plainly proved to be an antiseptic*. And this being the case, I cannot see how excrement can be reckoned one of the causes of such diseases as are of a putrid nature, and which must consequently derive their origin from a putrid cause.

But should we allow that the fœcal matter has some degree of septic power, we cannot even then with any appearance of reason conclude from thence that it should be more productive of putrid diseases in camps than in crowded cities, especially when we consider that it is always at some distance from the lines, and regularly covered up with earth every two or three days; whereas in several

* Experiment iii. Appendix.

crowded

crowded cities it is shamefully thrown out into the open streets.

After a putrid dysentery has once broke out in a camp, it is possible that the effluvia of the privies may contribute to spread the contagion; but how it should first produce the disease before any dysenteric excrement be lodged in the privies, is neither agreeable to reason nor the experiments just now related; and more especially in a northern climate, where, as I have already observed, the atmosphere is not easily contaminated on account of the constant ventilation.

What has been already observed concerning putrid excrement, intirely supersedes the necessity of adding much with regard to putrid urine, as they are much of the same nature. The steam however arising from the latter seems to be considerably more antiseptic than that arising from the former, according to several trials which I have made, and which would be superfluous to mention in this
 2 place,

place, especially as the same thing was observed by Sir John Pringle*, who in his second experiment says, that upon finding in urine a much greater quantity of volatile salt, and that more easily separable than in any other humour; and that stale urine is the least noxious of putrid animal substances, so far from dreading the volatile alkali as the deleterious part of corrupted bodies from this instance, we may rather infer it to be a sort of corrector of putrefaction.

From the whole, therefore, of what has now been observed concerning animal effluvia, this conclusion will naturally follow: That while a number of carcases of any kind are putrefying together, they may contaminate an atmosphere that is calm, warm, and moist; that this contagion may be propagated from one body or person to another by contact, breath, sweat, putrid dysenteric stools, &c. but that in an atmosphere without

* Appendix to the Observations, &c.

these

these requisites, a contagious principle diffused in this or any other manner, will either be soon intirely dispersed by the winds, or meeting with no fomes to cherish it, will become gradually weaker and weaker, till at last it lose all power of doing any harm.

C H A P. V.

Of vegetable Putrefaction; whether contagious?

SEVERAL authors of no small credit have alleged that putrid vegetable effluvia was the cause of intermittent and malignant distempers. Dr. Lind observes, that the English castle at Whydow is reckoned more unhealthful than the neighbouring places, as the sea breezes in coming to it always pass over an inconsiderable brook of water which produces some aquatic plants always covered with a putrid slime*. And Lancifus, in several parts of his works, blames the effluvia arising from steeped flax and hemp for being productive of what he calls a camp fever†.

* Essay on the Diseases incident to Europeans, &c.

† Nihilominus propter cannabis præcipue macerationem quæ multis abhinc annis propius illam urbem, quam olim, fieri, cœpta fuit. Ab Augusti principio ad exactum usque Septembrem adeo infestus evasit, ut per ea tempora multos cives et læserit et perdiderit. *Lancif. de nox. palud. effluv.*

It

It is not my business here to enquire how far putrid vegetable effluvia may be hurtful to animal life; either, perhaps, by rendering the air unfit for the purposes of respiration, or by other methods which we are possibly still less acquainted with. All that I propose is an attempt to shew whether this effluvia be a septic or an antiseptic; which I shall do by a few simple experiments. But before I proceed to these I must beg leave to observe, that it has been much litigated whether putrefaction be the same in the living and the dead animal; and after all, the matter remains still doubtful, and what I am by no means fit to determine; tho' I cannot help thinking, that at least they must be pretty nearly allied to each other; for we are assured by facts, that a putrefying dead animal, or a part of it, can communicate a putrid disease to a living one*.

* Sir John Pringle, *Observat. Journal de la contagion à Marscilles.* Summonte *Histor. di Napoli.*

In these cases then the putrid dead substance is the proximate cause of the disease; but we know no fact by which it is proved that any antiputrescent substance has been the proximate cause of a disease of the same nature.

But further; it would seem that the most learned and intelligent of our modern physicians have considered them in the same light; as they have always combated putrid distempers in the living subject, by such things as are found most antiseptic when applied to the dead one, as camphire, bark, &c.

And lastly, putrefaction is not only communicated from the dead to the living animal, but also from the living to the dead one; for a piece of meat sooner putrefies that has been breathed upon by a person who has diseased lungs and a bad breath, than another of the same weight that has been breathed upon for the same time by a sound person; and the sanies of a putrid ulcer applied to a piece of meat will make it putrefy
sooner

fooner than another piece of the same weight to which no such fancies has been applied.

But be this as it will, I know no author who has affirmed that any thing which is not septic can communicate a putrid disease; and therefore, if it should appear that neither the infusion nor effluvia of putrid vegetables is a septic, though we may perhaps justly accuse a putrefying mass of them as the cause of a variety of other disorders, yet we ought to be cautious how we accuse it as the cause of putrid ones.

EXPERIMENT XI.

Six small pieces of mutton, weighing two drachms each, were put into six tea-cups. The cups were filled up with pure water, and numbered 1, 2, 3, &c. Into number first was put one drachm of fresh leaves of *achillæa millefolium*; into number second, one drachm of the leaves of the *prunus spinosa*; into number third, one drachm of *hypericum*; into number

E fourth,

fourth, one drachm of taraxacum leontodon; into number fifth, one drachm of lactuca; into number sixth, one drachm of althæa officinalis. A seventh cup was filled with pure water, and two drachms of mutton put into it, which was designed as a standard.

They were all set together in a warm closet at 12 o'clock mid-day. The day following they were examined at the same hour, when the mutton in the pure water had fairly got the putrid smell, and tinged the liquor around it with a bloody colour. The water in all the other cups was also tinged a little, but in none of them nearly so much as in this, and number sixth only was just beginning to have a little of the putrid smell.

When they had stood 48 hours they were again examined. The water in all of them was now become more bloody. The standard cup smelled considerably more foetid than the day before, and number sixth hardly smelled better than it. All the others seemed now to be
nearly

nearly in the same state as the standard had been in at the end of 24 hours; only number first and number third rather smelled somewhat less foetid. But, as the whole of them were fairly become putrid, they were thrown out.

EXPERIMENT XII.

As it appears from the last experiment, that fresh vegetables have but a small degree of antiseptic power when they are put into the water at the same time with the meat which they are intended to preserve, I made a strong infusion in water of each of the above-mentioned vegetables, and kept all these infusions a month, by which time they were become highly putrid, and smelled disagreeably.

In this state their various antiseptic powers were compared with that of pure water, and found to be as follows. The infusion of achillæa mil-

E 2

lefolium

lesfolium preserved meat 12 days longer than pure water.

An infusion of the leaves of the *prunus spinosa* preserved it 17 days longer than water.

An infusion of *hypericum* preserved it 16 days longer than water.

An infusion of *taraxacum leontodon* preserved it 20 days longer than water.

An infusion of *lactuca* preserved it 11 days longer than water.

And an infusion of *althæa officinalis* preserved it 15 days longer than water.

Infusions of several other vegetables were tried; but when the vegetables were put into the water at the same time with the flesh, few of them preserved it above 24 hours longer than water. When the flesh was put into an infusion already become putrid, all of them preserved it much longer. Several of the last-mentioned trials were likewise repeated; and though the times which the infusions then preserved

preserved the flesh varied in some a few days from what I have set them down above, yet they all kept pretty nearly to what is there related, and the difference might perhaps arise from the different strength of the last infusions, which were all made without weight or measure; or it might arise from the variation of the weather, and other accidental circumstances.

By the foregoing experiments I had ascertained that vegetable infusions, after they had grown putrid, were so far from being septic, that they were even of a contrary nature. I resolved next to try whether a putrid vegetable substance could communicate putrefaction to a fresh animal one.

EXPERIMENT XIII.

With this intention, I reduced a quantity of cabbage leaves to a pulp in a mortar, and set them in a warm place to putrefy. A bowl of strawberries was likewise set along with

E 3 them.

them. When both the cabbage leaves and strawberries had undergone a fermentation, and now discovered by their smell that they were in that state in which vegetables are said to be putrid, I put a bit of mutton into each of them, and covered it over with the pulp. Another bit was put into a bowl of water for a standard, and they were all set together in a warm closet. The piece of mutton in the water had evidently turned putrid and tinged the water red in one night. Both that piece which was in the pulp of the cabbage, and that in the pulp of the strawberries, were kept three weeks, and continued perfectly sweet, and hard, as if they had been salted. The bowls were then overturned, and their contents lost by accident.

From these experiments it appears, that neither the putrid substance, nor the watery infusion of vegetables, is septic. Let us therefore proceed to examine putrid vegetable effluvia.

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EXPERIMENT XIV.

Two or three handfuls of different herbs, were put all together into a large tin decanter full of water. After this infusion had been kept about six weeks, was become fœtid, and crowded with animalcula, I suspended a piece of beef in the mouth of the decanter, so as to come as near the surface of the liquor as possible without touching it; and another piece in the mouth of another decanter of pure water, in the same manner. This last piece began to putrefy in about 30 hours; the other was perfectly sweet for six days, and then began to smell a little; but soon after grew so dry, that any further progress which the putrefaction might have made was thereby stopped.

This experiment was several times repeated, with little variation in the circumstances from what I have now mentioned.

I was therefore fully satisfied, by what is above related, that there was

nothing septic either in the infusion, putrid substance, or effluvia of putrid vegetables; and on that account thought it would be superfluous to endeavour to demonstrate it more fully by further trials.

I did not expect that an infusion of vegetable matter turned putrid would have been more antiseptic than a recent one, and suspected at first that something acid or alkaline might have been generated during the fermentation of the vegetable; but upon trial I could discover neither.

A strong vegetable infusion, which smelled extremely ill, and was taken from the decanter over which the meat in the last experiment had been suspended, did not grow turbid by the addition of corrosive sublimate.

It did not change the colour of syrup of violets, but only made it a little less transparent; which I suppose was owing to the black colour of the liquor poured into it.

It did not effervesce with the mineral nor vegetable acids.

Finding

Finding this to be the case, I suspected that it had undergone a sort of vinous fermentation. But, on comparing the rapid progress of the fermentation which had taken place in this infusion *, with the slow fermentation that generally takes place in the production of vinous liquors, I was inclined to doubt of this also; especially when I considered, that in this fermentation a foetid smell had begun to arise about the fifth or sixth day, and that no such smell arises neither during the time of, nor after the vinous fermentation. But, whatever be the cause of this antiseptic power in putrid vegetable infusions, it is certain that they are possessed of it. Without endeavouring therefore to determine the causes, it is sufficient for my purpose to have ascertained the fact, which, if I am not much mistaken, will hold pretty universally through the whole vegetable kingdom.

* This fermentation only lasted about ten days.

C H A P. VI.

*Of mineral Effluvia ; whether productive
of putrid Disorders?*

THERE are a great variety of minerals generated in the bowels of the earth, and some of these are the natural productions of almost every country, insomuch that there is hardly any kind of earth or stone that does not contain some share of them. But in this state of mixture with other bodies there is generally a small portion of them only exposed upon the surface of the earth. That part of them which is shut up in its caverns cannot send out any effluvia; and that part of them which is exposed on its surface is generally so blended with, and inveloped in other matter, that it can send out little or no effluvia.

For these reasons, though mineral effluvia has no great chance of proving hurtful to any great number of
the

the human race; yet there are particular employments, and even situations, which are peculiarly obnoxious to it. Such are miners, and others who, though not actually employed in working the mines, are settled in the neighbourhood of them.

It has been always observed, that miners, whatever metal they dig for, seldom live so long as other people. This may perhaps be owing to the mephitic air they so often meet with, which, though it agree in several properties with the air of the atmosphere, is, nevertheless, found by fatal experience to be totally unfit for the purposes of respiration. Or, supposing that they should not meet with mephitic air, yet the damp subterraneous places, in which so much of their time is spent, must inevitably contribute to shorten the duration of their existence. But, besides these causes, there are several of the minerals, as cobalt, and all the others that contain arsenic or mercury, whose effluvium is exceedingly hurtful to
the

the constitution. But, whether even these, though the most noxious kind of minerals, are productive of putrid diseases, is much to be doubted.

Those people who work much in mercury are generally lean, emaciated, and often die of consumptions and asthmas. They have also frequently rotten teeth and gums; which last complaint would make one suspect that mercury had something septic in its nature. But I do not recollect to have met with any account of putrid malignant diseases having destroyed more miners than other people, when they happened to attack those countries where mines were situated. Nay, Linnæus mentions a plague being stopped in Muscovy by mercury*.

Fallopian observes, that such as work in quick-silver mines seldom live above three or four years; that they are generally afflicted with palsies, vertigos, and other diseases of

* Amœnitat. Academ.

the nerves; that those who work in lead mines are liable to paralytic disorders, gripes, colics, and other complaints of the bowels †; but does not insinuate that they are more liable to putrid disorders than other people.

I do not chuse to dwell long upon this subject, as I never had an opportunity of living in the neighbourhood of mines, nor of observing the diseases of miners. I shall therefore conclude with observing, that, though mineral effluvia may be a predisposing cause of putrid diseases, by weakening the constitution and making it more liable to be overcome by contagion; yet I find no good authority to suspect that it can ever be the proximate one.

† Fallop. Opera.

C H A P. VII.

Of mixed Effluvia; whether septic or antiseptic?

UNDER the head of mixed effluvia, I comprehend that arising from marshes, lakes, and stagnating waters of all kinds, and from every other place where a variety of different substances are putrefying together.

The chief ingredients which constitute putrid marshes and lakes, are generally either animal or vegetable matter mixed with the water. There is seldom any thing mineral in this composition; but, without it, the two former are sufficient to give that disagreeable smell to these lakes which always arises from them in warm weather, and which, as it has always been reckoned a sign of putrefaction, gives birth to the opinion of their being reckoned the cause of putrid intermittents, dysenteries, &c.

Philosophers in all ages have with no small eagerness endeavoured to discover the causes of every natural phænomenon, and more particularly of those which happened most commonly, and in which they were most interested. And, as we are all deeply interested in any thing that affects health, the causes of diseases have in every age been investigated with a peculiar attention. But, as the secret springs which move and actuate the healthful body are infinitely removed beyond the sphere of our knowledge; so the causes which disorder it are often also either involved in inextricable difficulty, or so perplexed and complicated, that all the attainments we have hitherto made in science are not able to assist us sufficiently to unravel them.

To this perhaps it is owing, that, in endeavouring to account for epidemic disorders, we lay hold of every distinguishable difference which we can discover in the air, or situation of places that are attacked, from those
that

that are free ; and sometimes, without duly examining and considering these, conclude that we have discovered the cause of a disease, which is perhaps only known to the great Author of nature himself.

Thus, if an epidemic distemper rages in a camp or village, and this camp or village be in the neighbourhood of a foetid marsh, this marsh, as being most obviously different from any thing observable near the place, is immediately fixed upon as the cause of the distemper. I do not mean by this to assert, that the effluvia of a putrid marsh may not be unwholesome, that it may not cause diseases of various kinds ; but I am far from thinking that it can be the cause of those putrid ones for which it has been so often accused ; and I am even inclined to doubt of its insalubrity in any respect, when I consider that, when an army encamped in the neighbourhood of a marsh continues healthy, no notice is ever taken of such a marsh having been there :
Whereas,

Whereas, when sickness begins to rage, if there be any marsh near, it is immediately fixed upon as the cause. That this is frequently the case, appears from the relations of several military gentlemen with whom I have conversed on the subject, who, upon being desired to recollect the situations of the camps in which they had been, and the general state of health they enjoyed in them, have remembered to have lain in the proximity of several marshes where they were attacked with no epidemical distemper; and, on the other hand, to have been attacked with such distempers when they were not near any marsh.

But, lest I should be thought to have said too much upon this subject without giving any reasons in support of my opinion, I shall now relate a few experiments upon the marshes in the neighbourhood of this city (Edinburgh).

EXPERIMENT XV.

I caused a bason to be filled with some of the most foetid part of that marsh called the North-Loch *. Over this stinking matter I suspended a piece of beef, so as almost to touch its surface, and another piece of the same weight over a bason of pure water. On the third day, the piece over the pure water began to smell foetid, but that over the marshy matter was still sweet. They were both however by this time beginning to dry, and continued to do so in such a manner, that all further progress was thereby put to that putrefactive process which had already begun in one of them.

* The North-Loch lies in a hollow upon the north side of Edinburgh, and receives not only a great part of the filth of that city, but has likewise upon its banks the slaughter-houses, where almost all the cattle used by the inhabitants are killed.

EXPE-

EXPERIMENT XVI.

Not being able to draw any conclusive inference from the last experiment, as the bits of flesh made use of dried so fast, I took some pure water and diluted with it as much of this marshy matter as filled a large tea-cup: Into this I put two drachms of fresh mutton, and the same quantity into another cup of pure water for a standard. The cups were both placed together in the outside of a south window. The mutton in the common water soon began to tinge it of a bloody colour, and in about 24 hours a foetid smell arose from it. The marshy matter at this time was neither altered in the least from its original colour nor smell, and the mutton contained in it was perfectly sweet.

At the end of 48 hours the mutton in the common water had become considerably more putrid, and smelled more disagreeably than it had done the day before. That in the marshy matter only smelled of

the marsh, and, when it was washed off, smelled perfectly sweet.

The third day I took the mutton out of both the cups, washed each piece well with pure water, and found the piece that had lain in the marshy matter still sweet; the other, though less foetid after the washing, still had an ill smell, and had lost much of its solidity.

After six days more they were taken out again, and washed as before. The piece that had lain in the marshy matter was still solid, but began now to have a small degree of the putrid smell. The other piece was highly putrid and almost reduced to a jelly.

That which had lain in the marshy matter from this time began also to dissolve, but much more slowly than the other, and never acquired a strong putrid smell, nor ever altered the liquor in which it was infused from its original colour.

E X P E-

EXPERIMENT XVII.

As the last experiment had shewn that the marshy water had a considerable antiseptic power, I determined to try once more the effects of the effluvia arising from it; and for this purpose two pieces of beef of an equal weight were again suspended, one over a basin of marshy matter diluted as in the last experiment, and the other over a basin of pure water. Both the basins were now set in a damp place, to prevent the bits of meat from drying before the putrefactive process could come on. The piece over the marshy matter did not begin to putrefy till five days after the other.

EXPERIMENT XVIII.

Having by these trials satisfied myself concerning the antiseptic power of this marsh, *i. e.* the North-Loch,

and the effluvia arising from it*, I next got some of the putrid water from a ditch in the Meadow†: having filled a tea-cup with it and another with pure water, I put a bit of mutton into each cup, and set them together in the open air. The mutton in the pure water began to putrefy in about 36 hours. At the end of three days, that in the marsh water was quite sweet. On the fifth day it was taken out, washed carefully with pure water, and found perfectly sweet. That in the pure water was now become intolerably foetid, and on that account it was thrown away.

* As there is a large tannery upon the banks of the North-Loch, and large quantities of oak bark thrown out into it, it was objected to the above-mentioned experiments by an ingenious gentleman, that the antiseptic power of its water might proceed from this bark. To see whether this was the case, I repeated the experiments, and tried several more, with the water of this loch taken from a ditch on a higher ground than where the bark lay, and to which no part of its virtues could ever come, and found that this water was as antiseptic as the former.

† The Meadow is a piece of low ground on the south side of Edinburgh, drained by ditches, which in the summer contain in some places an extremely putrid stagnating water.

The

The seventh day the mutton in the marsh water was washed again, and found as fresh as before. When it had lain in it about six weeks, it still continued perfectly sweet, and the liquor around it of the same smell and colour as at first. After two months, things were exactly the same. The mutton was then thrown out.

EXPERIMENT XIX.

Some water was taken out of a gravel pit in which several dead dogs and other animals and vegetables were putrefying. This water, while in the pit, was over-grown with a green crust, and crowded with animalcula, many of which were observable by the naked eye, and myriads which escaped the naked eye were easily detected by the microscope. It had, however, little or no putrid smell.

A piece of fresh mutton was put into a gallypot, and some of this water poured over it; and another into a

gallypot which contained only pure water. They were both exposed to the sun. The piece in the pure water began to putrefy the second day: that in the gravel-pit water did not begin to putrefy till the fifth.

On repeating this experiment, the piece of mutton in the gravel-pit water did not begin to putrefy till the eighth day; that in the pure water began to do so towards the close of the second.

EXPERIMENT XX.

A piece of fresh mutton was put into some water taken from a marsh between the Abbey of Holyroodhouse and Salisbury rocks*, and another into pure water: They were both set on the outside of a window, as before. Towards the end of the second day, the mutton in the pure water began to grow

* This marsh is in some parts extremely foetid, being a reservoir for the filth that runs from the south side of the Canongate and several other parts of the suburbs of Edinburgh.

foetid;

fœtid; on the sixth, that in the marsh water, seeming perfectly sweet, was taken out and carefully washed, to prevent any deception; when it was found not only intirely free from any ill smell, but likewise as solid as if it had been in salt brine; although it was now warm and sometimes moist weather, being the latter end of July and beginning of August.

This piece did not begin to lose its solidity till it had stood near six weeks, and even then, when it was thrown away, had scarcely any fœtid smell.

EXPERIMENT XXI.

Having now tried the water of all the marshes in the neighbourhood of Edinburgh, I next procured some from a marsh in the country, into which no dead animals were thrown, and which nevertheless smelled extremely ill. A piece of fresh beef was put into some of this, and another into pure water. That in the pure water began to turn putrid on the third day. That in this marsh water did not begin

gin to do so till it had lain there near two months*.

The marsh miasmata, or effluvia arising from marshes, as I have hinted before, has always been considered as one of the most common and inveterate causes of the putrid intermittents and dysenteries, that so frequently attack armies in long encampments. The above experiments plainly shew, that not only the marsh waters themselves, but also the effluvia arising from them, are antiseptic. This then being proved, I think it will be extremely difficult to shew how they can be productive of diseases which are evidently of a septic nature.

It is far from being an easy matter, nay it is often an impossible one, to account for some of the phenomena that from time to time present themselves to us. This of marsh water preserving meat so long, though no

* Since I have been in London, I have likewise tried water from the canal in St. James's Park, from Fleet Ditch, and found that they preserved meat much longer than pure water.

person would have deduced it from reasoning *a priori*, seems nevertheless, when the fact is known, to admit of a pretty natural explication. The causes then of its becoming antiseptic may be these three: First, the great quantity of vegetable matter infused in almost every marsh, which is a strong antiseptic, as has been already abundantly proved in the preceding experiments upon vegetables. Secondly, from the volatile salt generated in all putrid substances, which salt, though not easily detected in marsh water, may nevertheless exist there in small quantities. And thirdly, because putrid marsh water has already undergone that species of fermentation which is previously necessary to, and the immediate forerunner of, putrefaction; and therefore, by an uniform law of nature, can never undergo it again.

Among the more rude and barbarous nations we frequently meet with customs which at first view seem totally repugnant and irreconcilable to reason ;

reason; and yet, upon considering them more attentively, we generally find that they have some foundation in Nature, and have taken their rise from experience and observation. Thus we are told by Alexander Benedictus, that a physician among the Tartars, in the time of a severe plague, ordered all the dogs to be killed and thrown into the most public streets and roads, that the atmosphere might be filled with a putrid smell; by which means, he says, the people were restored to health, and that they continue still the same practice in the like cases*. And, similar to this, we are also informed by Gregorius Pictorius, that he had heard some person affirm, that, in the time of an epidemic infection, nothing was better or more salutary, than for every one to smell three times a day, either a necessary-house or a sheep-house†. Is it possible that

* Alexand. Benedict. de Peste, cap. 6.

† Gregor. Pictor. Dialog. 2. de bon. Valetudin.

these customs, seemingly so contradictory to reason, could arise from chance; were they not rather deduced from observations similar to those above related concerning excrement and marsh water?

Whether a practice of this kind would be attended with advantage or disadvantage in time of general infection, I shall not pretend to determine. The former, however, is at least possible; and therefore, as we have but so limited a knowledge of the action and effect of bodies on the human constitution, these relations seem not to deserve that ridicule which Lancisi has thrown out against them? which, I suppose, he did because they thwarted his hypothesis concerning the noxious qualities of the marsh miasmata.

I do not mean here to plead the innocence of marsh miasmata, or to affirm that marshes are salutary, because I have found the water of them to be antiseptic. I am not ignorant that

that almost all the authors who have treated on this subject, and especially Lancisi, have agreed that it was hurtful, and adduced many instances of cities and armies having been attacked with putrid malignant diseases when exposed to it. Nevertheless, it has likewise been observed, that armies have sometimes escaped healthful from the neighbourhood of a marsh; which, together with the experiments I have above related, are, I imagine, sufficient to render its noxious qualities a little doubtful, at least till the matter is better cleared up by further experiments, and the observations of such people as are well acquainted both with marshy and dry situations, and the consequences of living in them.

But, though we should even allow that the exhalations arising from marshes are not septic; they may nevertheless be unsalutary, as they abound with humidity arising from the marsh, with which the atmosphere in the neighbourhood of it must

must be loaded, and by which the perspiration must be diminished, and thereby a variety of diseases produced; and this is no more than what would happen from a lake of pure water, especially if it were shallow, as that is the condition of all others the most favourable to evaporation. Whoever reads the observations of Lancisis with attention, will, I imagine, be convinced of what I have here said; for the greatest part of the putrid epidemics which he mentions, always followed remarkable inundations of the Tyber, and several of them sooner than the water left by those inundations could have become putrid; so that all, or the greater part of the putrid diseases ascribed by Lancisis to marsh miasmata, may be fairly attributed to the effect of moisture alone, independent of any mixture of putrid effluvia.

Lancisis seems in several places to have hinted, that the insalubrity of marshes might be owing to that vast number and variety of animalcula
which

which are bred in them, and of insects which are floating upon and flying about them ; but it will appear afterwards, that neither animalcula nor insects have any power to bring on putrefaction, or to accelerate its progress after it is brought on. So that, from the whole of what has been observed concerning stagnating marshes, it seems hardly to be credited that they can be the cause of all those distempers which have generally been attributed to them* ; at least, a further enquiry into their nature and effects, in order to set this matter in a clearer light, will be necessary, before we can finally decide either with regard to their noxious or innoxious qualities.

* If the effluvia arising from putrid marshes has such a power of bringing intermittents, dysenteries, &c. upon those people who live, or are encamped in their neighbourhood, why are not the labourers who are employed in draining of bogs and marshes also affected with these distempers? And why were not some of the people who have been this summer employed in cleaning the canal in St. James's Park affected? which has not happened; for, upon enquiry, I find that they all were in good health during the time they were employed in that work.

I would

I would therefore recommend to those gentlemen who are, or may be, situated near marshes, which are supposed to spread infection in their neighbourhood, to observe, and endeavour to discover, whether such infection be owing to these marshes, or to other causes. If it be not owing to them, the world will thereby be extricated from a vulgar error of long standing: if it be owing to them, we shall become more able to guard against the danger, by knowing certainly from whence it arises.

G

CHAP.

C H A P. VIII.

*Of animalcula: whether the cause or effect
of putrefaction?*

AS putrid epidemic diseases have generally been observed to rage with the greatest violence, in those seasons which have been remarkably productive of animalcula and insects, the ingenious Kircher, from thence, first formed the hypothesis of animalcula being the cause of putrefaction*; in which he was followed by the celebrated Linnæus†, by Marc. Antonin. Plenciz‡, and by several other authors of reputation.

As this hypothesis therefore has been adopted, and defended by so eminent a naturalist as Linnæus; as it has attracted the attention of several others, who have been peculiarly at-

* Kircher de caus. et effect. pestis.

† Ex allatis, quas passim afferunt auctores, causis, ea nobis videtur proxima, qua statuitur contagium ex vivis animalculis provenire. *Amœnitat. Academ.*

‡ Marc. Antonin. Plenciz. *opera med. physic.*

tentive to the minutiae of nature * ; I shall examine it the more carefully : neither shall I draw any conclusions from this examination, but such as result naturally from the experiments to be mentioned in the course of it.

As Kircher and his followers have asserted, that animalcula are the cause of putrefaction ; so, on the other hand, many others had suspected, that the animalcula we generally meet with in putrid substances, were rather the effect than the cause of putrefaction ; and that putrefying matter only afforded a proper nidus for the evolution of the ova of animalcula †, after these ova were deposited in it, either by the parent animals, or by the air ‡ : I therefore determined to satisfy myself concerning the truth of what had been asserted by Redi and Arezzo ; viz. whether animal and vegetable matter could become putrid without producing animalcula, when flies, and

* Needham and Buffon.

† Redi degli animali viventi, negli animali viventi.

‡ Arezzo.

other insects, were prohibited from laying their eggs in it? But, previous to this, I thought it would not be amiss to try if a piece of flesh could putrefy when closely shut up, without the air having any access to it; and what phænomena it would exhibit in these circumstances.

EXPERIMENT XXII.

I cut a small piece out of the middle of a large loin of beef, that I might have it without any of the seeds of animalcula in it. This I crammed, as hard as I could, into a small phial glass: when it was quite full, I corked it hard, secured the cork with a packthread, and sealed it with wax. Thus prepared, it was set in a warm closet, about the latter end of April. On the seventh of the following July, having cut the packthread, with a design to open the phial, the cork immediately burst out with some force; and part of the meat, with a little fixed air, issued out after it, with such an intolerably putrid smell as struck me almost

almost instantaneously with a head-ach, and obliged me to run and deposite the phial under the chimney, that the putrid smell might escape. This being done, I opened the windows; and, having thereby cleared the room a little of the smell, I took a small bit of the beef out of the phial, and put it into some pure water, which had been examined, and found to contain no animalcula. Having mixed this putrid pulp of beef and the water together, and allowed them to stand a few minutes, that the pulp might subside, I examined several quantities of the water with all the different glasses of Wilson's microscope*, but could discover no animalcula in it. Some of this water was put into a phial; the phial was closely corked, and the water kept three days: being then examined, no animalcula could be discovered in it. The phial

* Wilson's microscope was what I used in all the following experiments. In order to fit it for examining aquatic objects, I had a small watch-glass ground down, so as to go into it; and into this I poured the water which I wanted to examine.

was then opened, and set in the outside of a window, where it remained several days, and the water in it was examined from time to time, but no animalcula were ever discovered in it.

Some of the water was likewise kept in a tea-cup in the open air, and never contained any animalcula.

Though the smell emitted from this phial was almost intolerable, yet the beef had hardly lost any thing of its original fresh colour, and several pieces of it even retained something of their original firmness.

EXPERIMENT XXIII.

A piece of fresh beef was put into a gallypot full of water, and a piece of thin silk tied over it, as mentioned by Redi. Whether any insects, or their ova, got through this, I know not; but in five or six days it was crouded with a variety of animalcula.

EXPERIMENT XXIV.

The same experiment was repeated, with a piece of thicker silk tied over the
the

the gallypot. Several animalcula were observed in this, though not near so many as in the other.

EXPERIMENT XXV.

The same experiment was repeated, with a piece of sheep's skin tied over the mouth of the gallypot. In this no animalcula could be discovered: by which it appears, that the ova of animalcula are extremely minute, and not easily prevented from finding entrance into a proper nidus for their future evolution.

These experiments led me one single step forward, in what I had apprehended to be the origin of the animalcula found in putrid matter: they had shewn me, that a part of an animal substance, in which there were none of the ova of animalcula, when excluded from any communication with the open air, could putrefy without producing any of these creatures.

EXPERIMENT XXVI.

But, as animalcula seem to be more numerous in putrefying vegetable,

than in putrefying animal matter, I resolved to see what phenomena a piece of vegetable matter would exhibit when treated as in the 22d experiment; and therefore cut two small pieces out of the heart of a solid turnip, divided them into two equal parts, put each of these parts into a phial glass; filled up both the glasses with water that had been boiled about an hour before. One of them I corked and sealed; the other I left uncorked. When they had both stood about eight days in the outside of a window, the infusion in the uncorked glass was crouded with animalcula. On opening the other, and examining its contents, none could be discovered in it; though both these and the former had exactly that smell which is peculiar to putrid vegetables. This experiment led me one step farther, and shewed me, that vegetable substances, when they contained none of the seeds of animalcula, and were excluded from all communication with the atmosphere, likewise putrefied

putrefied without producing any living creature.

But as we, perhaps, neither know the utmost possible limits of the greatness or littleness of animals, it has been objected by some, that our not being able, even with the best glasses, to discover animalcula in any thing putrid, is no positive proof that they do not exist there*. In answer to this, I shall only observe, that, as animalcula are generally easily discovered, even with a glass which magnifies but little, and often by the naked eye, in almost all substances that have putrefied in the open air, I cannot see any reason to suspect, that, in those things which putrefy while they are excluded from all communication with the air, they should be so extremely minute, as to elude our keen-

* Sed hoc adversariis nostris concedere non possumus, quod putredo absque tali materia animata esse possit, quæ materia animata saltem armatis oculis detegi potest. Interim, quamvis selectissimis etiam microscopiis, talis aliquando non detegeretur, exinde tamen ejusdem absentia inferri non potest. Marc. Anton. Plenciz. *opera medico-physica.*

est endeavours to detect them even with the best glasses.

EXPERIMENT XXVII.

Having now succeeded so far as to have satisfied myself, that a part, either of an animal, or a vegetable, that did not contain any of the ova of animalcula, could, when closely shut up, put on all the appearances of putrefaction, without producing any thing living, I now resolved to try likewise, whether in a vegetable, which I was certain did contain these ova, they could be hatched without the assistance of any other air than that contained in the water in which the vegetable was infused; and whether they could be hatched, when the infusion had a communication with a small quantity of air, but not with the whole atmosphere.

For this purpose, an ounce of the *Leontodon Taraxacum* was cut small, divided into three equal parts, and each of these parts put into a six ounce phial. One of the phials was filled up

with pure water, and closely corked; so that no vacuity was left between the cork and the surface of the water. The other phial had just as much water put into it as covered the herb, and was closely corked also. The third was filled with pure water only, and was left uncorked, as a standard. I did not examine any of them till they had stood five days in a warm closet. In the first and second phial I could discover no animalcula; but in the third, or standard one, they were swarming in prodigious numbers. The first and second phials were then corked again as before, and set in the same place, where they remained five days more; but still no animalcula were generated in them. They were then left uncorked, and set in the open air, where they remained several days; and were examined almost every day, without ever discovering the smallest appearance of any thing living. Afterwards, some fresh leaves of the *Leontodon* were put into each phial: but,

but, even by this addition, no animalcula were ever produced in them.

This experiment was repeated several times, with different herbs; but no animalcula were ever produced, when the infusions were closely shut up: nor ever but once or twice in the infusions that had once been shut up for some days, though again exposed to the open air, and tried with additions of fresh vegetables, in which I was certain that the ova of animalcula were contained; so that it would seem, that though animalcula, after they are hatched, can live in a vegetable infusion that has undergone a kind of fermentation, yet such an infusion is unfavourable for hatching or evolving them from their ova.

Though so many trials of this kind afforded almost a convincing proof, that the animalcula contained in putrid substances were generated from their ova, either deposited there by the parent animals, or the circulating air; yet I resolved, if possible, to have still further satisfaction on that head: not
that

that I believed the possibility of a spontaneous generation, already so long and justly exploded ; but that, by investigating the manner how animalcula are bred in putrid matter, I might thereby the better understand, whether they are accidental or essential to it. Accordingly, during the frosty weather in winter, I made a variety of infusions, both of animal and vegetable substances. The animal substances all at length dissolved, though more slowly than in summer, and had every one of the usual appearances of putrefaction, except animalcula. The vegetable substances too, had all the symptoms of vegetable putrefaction ; but no animalcula were ever produced in them when the weather was frosty, and seldom even in the mildest months of winter.

By all these trials, I was now fully confirmed in the opinion, that the animalcula found in putrid matter were only generated there from their ova, deposited *ab extra*, and not from any living organic parts, according to Buffon ;

fon * ; nor from their stamina or rudiments necessarily preexisting in every animal and vegetable substance, according to Marc. Anton. Plenciz†. This opinion I was glad to find corroborated by several gentlemen of learning, and knowledge of natural history, and particularly by the celebrated Redi, one of whose experiments I cannot help here taking notice of. “ Il giorno 4 di marzo pestato nel mortajo di marmo con pestello di legno: una buona quantità di Giacenti Turchini, la divisi in quattro parti; due parti ne riposi in due vasi di vetro, e li lasciai aperti senza coprirli con cosa veruna: l'altre due parti le distribui in due caraffe, e col catone turata la bocca del loro collo la riscoperci con carta, e la fermai con buona legatura; e tutti a quattro questi vasi gli collocai insieme in una stessa stanza voltata a mezzo giorno sopra una stessa tavola. Dentro le due caraffe ferrate non

* Buffon *Histoire Naturelle*.

† Marc. Anton. Plenciz. *opera med. physc.*

ho mai veduto nascere alcun verme, ne alcuna farfalla, ne altro animalletto volante, per non avere a rinovarło a volta per volta in tutte, l' altre sequenti esperienze di questo diario, dico di nuovo, che lo stesso, costantemente e sempre avvenuto in tutti gli alteri fiori pesti, che ho tenuti in vasi di vetro ferrati, ed ogni prova, che ho fatta l' ho fatta sempre ugualmente a doppio, e in vasi ferrati, e in vasi aperti *."

By some of the experiments now related, as well as by this of Redi, and by what will appear afterward, it would seem that there are several other things necessary towards the evolution of animalcula, besides the mere existence of their ova deposited in a proper nidus. The agency of the circulating air, or at least of some part of it, is requisite, in order to hatch and communicate a vivifying principle to them. And a small, or even pretty large, quantity of the air, that does not communicate with the atmosphere, seems insufficient for this

* Redi degli animali viventi, negli animali viventi.

purpose;

purpose * ; for one of the phials, with the infusion of *Leontodon*, had only about a fifth part of its space filled by the water and plant, and yet no animalcula were produced in it. Heat too must be joined to this circulating air, otherwise the ova cannot become living creatures ; but this heat must be various, for the generation of the various species of beings to be evolved.

When we seriously consider these things, we cannot help concluding, that, if animalcula were the cause of putrefaction, then putrefaction could only happen in places where the ova of animalcula had access to the body that was to become putrid ; whereas it has been proved to happen more easily in an air pump nearly exhausted, into which hardly any ova could find entrance, than in the open air, where

* Perhaps no quantity of air, if it does not communicate with the atmosphere, will answer the purpose of hatching animalcula. I have since infused an ounce of *Leontodon* with two ounces of water, in a bottle that held four gallons, which being closely shut up, no animalcula were produced.

myriads

myriads of them might be deposited at once *. It could not then happen in confined air ; for we have seen that animalcula cannot be hatched there. But, by an experiment already mentioned, it happened in a phial full of beef, most carefully shut up, where any particles of air that were left must have been in an extremely confined state. Nor could it then happen in very cold weather ; for it has already been demonstrated, that no animalcula are hatched in the colder months of winter ; whereas it is well known, that meat, without salt, will in time become putrid, even in the coldest of them ; and that it will putrefy nearly as soon in 98 degrees of artificial heat in winter, as in the same degree of natural heat in the summer, though in the latter case it will produce animalcula, and not in the former ; which surely can be owing to no other cause than that, in the summer, the ova of animalcula are deposited almost every where by the parent ani-

* Macbride *Exper.* 18, 19, 20.

mals, and by the air ; and that, in the winter, these animals are either dead or asleep, and consequently no ova are any where to be met with. This opinion I am the more confirmed in, when I reflect, that, in making experiments with putrid substances, which I have been engaged in these several years, I have always found that fewer animalcula were produced, and with greater difficulty, in the beginning of the summer, before the air was croud-ed with insects, than in the latter end of it, when they had multiplied amazingly, though the degrees of heat happened to be nearly equal at both these times of the year.

But further; the doctrine of animalcula being the cause of putrefaction, will, unavoidably, run the supporters of it into the absurdities of equivocal or spontaneous generation, which some of them indeed have made no scruple of asserting*: For we have already shewn, that a part of an animal or vegetable, which contained

* Omne animal ex putredine nascitur. Kircher *de caus. pest.*

none of the seeds of animalcula, grew putrid, when it was so closely shut up that none of these seeds could possibly find admittance to it. Now, if animalcula existed here, they must have been generated spontaneously; but such a method of generation has been demonstrated, by the ablest philosophers, to derogate from the power of the Deity, and be incompatible with his perfections.

Those who maintain, that animalcula are the cause of putrefaction, say, that the foetid smell, arising from putrid matter, is an effect of the excrement of these animalcula *. But this I much suspect, for several reasons: First, because it has been already proved, that putrefaction really exists, and is attended with a foetid smell, without any animalcula; which could not possibly happen, if this foetor depended

* Vid. Lowenh. *arcan. natur. detect.* & Plenciz *Tractat. de contagio*, cujus hæc sunt verba. Quare eadem ratione qua obviis nostris sensibus videmus aquas paludosas ab his turbidas et male olentes reddi, admittere pariter debemus, alia liquida, si putrescant ab insensibilibus animalculis, eorum excrementis, et ovulis, turbida et foetida evenire.

upon their excrement. Secondly, because, even in those fluids where animalcula existed, I have never observed the smell to be any way proportioned to their number. And, thirdly, because I have seen water that had been long kept in cisterns amazingly full of animalcula, though it was transparent, and smelled perfectly sweet.

EXPERIMENT XXVIII.

Though the proofs I had already obtained would, perhaps, have convinced any one less sceptic in philosophical enquiries, that animalcula were only an effect, and not the cause of putrefaction, I resolved to prosecute my experiments still farther, and try every thing which I thought had the smallest tendency either to prove or disprove this doctrine. Therefore, in order to see whether putrefaction would take place sooner in a piece of meat immersed into a liquid which was crouded with animalcula, than in another liquid which contained none, I divided an ounce of fresh
mutton

mutton into two equal parts. One of these parts was put into a gallypot, and some pure water poured over it; the other was also put into a gallypot along with some of an infusion of Leontodon, which had been kept about a month, and was amazingly full of animalcula. Both the gallypots were then set in a close warm room, where they remained 48 hours. On examining them, the piece of mutton in the fountain water was become considerably putrid, smelled disagreeably, and had tinged the water about it with a dirty blood-colour, something like the washings of meat; but no animalcula could be discovered in it.

The other piece of mutton, that was covered with the infusion of Leontodon, seemed hardly to have suffered any change; neither was the liquor around it in the least bloody, or altered from its original colour; and the whole smelled exactly as the infusion had done before the mutton was put into it. On examining some of this liquor with the microscope, not

one of all that amazing number of animalcula, which had formerly existed in it, could now be observed.

As I had not suspected that the animalcula would have been destroyed in this experiment, that I might satisfy myself whether the mutton had been the cause of their destruction, I let fall a few drops of a putrid infusion, much crowded with animalcula, into the liquor, then in the concave glass of the microscope, which was that in which I supposed the mutton had destroyed the animalcula. On viewing this mixture, as soon as I could bring a glass to a proper focus, a large quantity of animalcula were moving briskly about in it. On observing them attentively, their motion soon became more languid, and, gradually decreasing, entirely ceased in about three minutes. I then mixed half an ounce of the simple infusion of the *Leontodon*, with the same quantity of that infusion of mutton which had been made in pure water. On examining

mining a part of this mixture as fast as I could, it was crouded with lively animalcula. On examining a little more of it, about half an hour after, not one could be discovered in it. Both the gallypots, with their contents, were now set on the outside of a south window. After they had remained there about a week, a few animalcula were discovered in each; but they seemed to be of a different kind, and not near so brisk as those which existed in the simple infusion of the Leontodon, before it was poured on the mutton.

On examining now the two pieces of mutton, that in the infusion of the herb still continued perfectly sweet and solid; the other, in the pure water, was almost totally dissolved into a nauseous putrid mass.

This antiseptic power of the infusion of Leontodon, though amazingly full of animalcula, put me upon trying infusions of several other green vegetables; all of which, without any exception, produced more or fewer animal-

cula, and all were more or less antiseptic, though this antiseptic power seemed to be no way regulated by the number of animalcula existing in the infusion; for I several times found, that an infusion, crowded with these animalcula, was a much stronger antiseptic than another, which contained but few of them; et vice versa.

EXPERIMENT XXIX.

By the last experiment, I had found, that even a number of animalcula, as large as could well be conceived, when existing in a vegetable infusion, had not in the least accelerated putrefaction; but that, on the contrary, the infusion, whether owing to them, or to some other cause, had powerfully resisted it. I resolved, therefore, in order to satisfy myself further concerning their agency in the process of putrefaction, to see if I could produce them in a liquid, without making it undergo this process.

With this intention, I infused some of the fresh leaves of *Leontodon* in
pure

pure water, which had been boiled a little before. When they had lain in it one night, I rubbed them all carefully against each other, with a view to brush off from them into the water the eggs of the animalcula, which might be adhering to them. This done, the water was strained through a thin cloth, put into an open phial glass, and exposed to the sun. After it had stood two days in this situation, a considerable number of animalcula were observed in it. They increased daily, and in ten days were become almost as numerous as I had ever observed them in any vegetable infusion. The water, during this time, had deposited an exceeding small quantity of a kind of filamentary sediment, but continued always perfectly transparent, nor ever had the least disagreeable smell.

By the 28th experiment it appeared, that these animalcula, which were produced in a vegetable infusion, had no power to communicate putrefaction to an animal substance; and by this
it

it seemed plain, that these creatures could be generated easily in water, when their ova were deposited in it, without the necessity of the water going through any previous fermentation. It may, therefore, I think, safely be inferred from this, that the animalcula produced by vegetable putrefaction, are not the cause of animal putrefaction; and that, as they can be evolved from their ova, without any putrefaction, they are, therefore, not essential, but accidental to it.

EXPERIMENT XXX.

As I had now found that putrid vegetable matter did not contaminate fresh animal matter, I also resolved to try whether putrid animal matter would affect fresh vegetable matter.

For this purpose, I put some pieces of fresh *Leontodon* into a cup of pure water, and some other pieces, taken from the same plant, into another cup of putrid infusion of mutton. After they had stood together about ten days, I took the pieces of *Leontodon*
out

out of the putrid animal infusion, and, having washed them carefully with water, found that they looked as fresh, and smelled as sweet, as the first day they were put into the infusion. The other pieces, likewise, in the water, were, as yet, perfectly sweet; but, by long keeping, began, at last, to dissolve; as did also those in the putrid animal liquor, both, as nearly as I could discover, about the same time.

EXPERIMENT XXXI.

I resolved next to try, whether putrid vegetables had any power to accelerate the putrefaction of fresh ones. Accordingly, a piece of lactuca virofa, newly taken from the ground, was covered over with putrid strawberries. After twenty days, being taken out and washed, it was as fresh as at first. It was then put again into the same putrid mash, where, after it had lain a month more, it was still fresh, and the texture of its parts nothing injured. Another piece of the same plant was put into a putrid infusion

sion of *Leontodon*, and was taken out nearly as green and solid as when put in, after having remained there about six weeks; while a third piece, which had been put, at the same time, into pure water, was now found much discoloured, and almost totally dissolved.

Notwithstanding that the lactuca continued so long fresh, when covered with putrid vegetable matter, a cabbage leaf, put into a parcel of fermenting garden weeds, began, in a few days, to lose its colour, and run into the same state; but, perhaps, if these weeds had then gone through their fermentative state, and been in a state of real putrefaction, the cabbage leaf might have been longer preserved by them.

EXPERIMENT XXXII.

That putrid animal substances, when they come into contact with fresh ones, accelerate their putrefaction, is a fact already well known; but I did not know, whether the animalcula generated

rated in these putrid animal substances would have the same effect. I therefore collected a parcel of maggots, from a piece of putrid mutton, and put them into a thin cloth; by pouring fresh water repeatedly upon them, I, at last, got them intirely cleared of all their fordes, and even of their putrid smell. I then put a piece of fresh mutton into a pill box, which I had bored in the lid to admit the air, and put the maggots to it. Another piece of mutton, alone, was put into another box of the same kind, to serve as a standard. On examining the box with the maggots, about twelve hours after, some of them seemed to be dead, and others to be preying upon the mutton; but no ill smell arose from them. About 18 hours after this examination, I first observed a fœtor begin to issue from the box with the mutton and animalcula; and, on opening it, found nearly one half of them seemingly dead, and the rest crawling about briskly upon the mutton. The piece of mutton in
the

the other box did not begin to emit a foetid smell, till about 15 hours after ; but the weather, during this experiment, was rather cold, though it was in the beginning of August.

I do not chuse to draw any conclusion from this experiment, as it was never repeated on account of its disagreeable nature.

EXPERIMENT XXXIII.

As I had found, by the preceding experiments, that animalcula could not be produced without some communication with the external air, I now proceeded to try, if those which were already generated could continue their existence without any communication with it. For this purpose, I took some of an infusion of rumex acetosa, which was crouded with animalcula ; and, having filled a small phial with it, corked it up hard, leaving no empty space between the cork and the infusion. This done, it was set in the outside of a window, in the evening, and examined next morning, about
ten

ten o'clock; but, after several repeated trials, nothing living could be discovered in it; though the dead bodies of the animalcula, which had formerly existed there, were plainly to be seen.

EXPERIMENT XXXIV.

Another small phial was now filled with an infusion of mutton, and corked as the last, being also set in the outside of a window: it was examined, after having stood there two hours, and found as full of lively animalcula as before it was put into the phial. I had not leisure to examine it again, till about ten hours more had elapsed, when they were all dead. This last examination was made with candle-light. Lest I should have been deceived by this method, it was repeated again next morning; but still nothing living could be discovered.

EXPERIMENT XXXV.

As I had not been able to hatch animalcula in a closely corked phial,
though

though it was more than two-thirds full of air, I resolved to try, whether those, already alive, could continue their existence in a phial of this kind. Accordingly, I put about two drachms of the infusion of mutton, made use of in the last experiment, into an ounce phial, and corked the phial as closely as I could. This was done about mid-day, and the contents of the phial were examined in the evening, when the animalcula were all as lively as before they were put into the phial. The next morning they continued still lively. I then filled the phial quite full, and corked it. In the evening the liquor was examined again, when the motion of the animalcula appeared something more languid than at the preceding examinations. The following morning, they seemed as brisk again as ever. I then corked and sealed the mouth of the phial, and allowed it to stand two days; after which, I found them still alive and brisk. The phial was then corked again, but not sealed, and examined after

after about sixteen hours, when they were all dead. Whether this was accidental, or whether the natural period of their existence was arrived, I shall not positively determine; though I think the latter could hardly be the case, as they continued still alive in the infusion of mutton, from whence that which filled this phial was taken *.

Notwithstanding what I have now related, as it is asserted both by Buffon and Needham, that animalcula, or living organized bodies, as Buffon calls them, were generated in almost every animal and vegetable substance, though inclosed in phials, and excluded, in the exactest manner, from the air †. I resolved to push these experiments a little further, which was done as follows :

E X.

* It is possible there may have been a rent in this phial, by which air was admitted into it, for I have several times since tried this experiment, and the animalcula never lived above 24 hours.

† J'étois donc assuré par les expériences que je viens de rapporter, que les femelles ont, comme le mâles, une liqueur féminale qui contient des corps en mouvement :

EXPERIMENT XXXVI.

An infusion of pepper in water being mentioned by Buffon, and several others, as one of those things that are most fruitfully productive of animalcula, I put a little of it into a six ounce phial, and having filled the phial up with pure water, corked it as firmly as I could. This infusion was care-

Je m'étois confirmé de plus en plus dans l'opinion, que ces corps en mouvement ne sont pas de vrais animaux, mais seulement des parties organiques vivantes : Je m'étois convaincu que ces parties existent non seulement dans les liqueurs seminales des deux sexes, mais dans la chair même des animaux, & dans les germes des végétaux ; & pour reconnoître si toutes les parties des animaux, & toutes les germes des végétaux contenoient aussi des parties organiques vivantes, je fis faire des infusions de la chair de différens animaux, & de plus de vingt especes de graines de différens plantes. Je mis cette chair & ces graines dans de petites bouteilles exactement bouchées, dans lesquelles je mettois assez d'eau pour recouvrir d'une demipouce environ les chairs, ou les graines ; & les ayant ensuite observées quatre ou cinq jours après les avoir mises en infusion, j'eus la satisfaction de trouver dans toutes, ces mêmes parties organiques en mouvement ; les unes paroissoient plus tôt, les autres plus tard, quelques unes conservoient leur mouvement pendant des mois entières, d'autres cessoient plus tôt ; les unes produisoient d'abord de gros globules en mouvement, qu'on auroit pris pour des animaux, & qui changeoient de figure, se separoient, & devenoient successivement plus petits.

Buffon *Histoire Naturelle*, tom. ii. p. 255. 4to.

fully

fully examined every day, for the space of eight days, with all the different glasses I had ; but no animalcula, nor any thing resembling the living organized bodies of Buffon, could ever be observed in it. It was then exposed uncorked to the open air, and examined every two or three days, for the space of a month ; but no living thing was ever generated in it.

EXPERIMENT XXXVII.

As I could hardly believe that so expert a naturalist as Buffon could be mistaken in what he asserts to have seen such a repeated number of times, that I might satisfy myself still further, I infused several small quantities of pepper into several phials of water, some of which were full, others half full, and some again one third full ; yet no animalcula could ever be discovered in any of them, nor any thing that had any motion, but what was impressed upon it *ab extra*.

These phials were kept closely corked two months : on opening them,

and keeping them a week longer, some animalcula were generated in them.

EXPERIMENT XXXVIII.

I next tried the jelly which came from several sorts of roasted meat, which, when I left it in the open air, never failed to be crowded, in a few days, with animalcula; but, whenever it was closely shut up in phials, whether they were full, half full, or in any degree from that downwards, none could ever be observed in it, by the severest scrutiny, though some of the phials were kept several months.

In this fruitless enquiry, as I had never been able to detect these self-moving particles, or *corps organiques vivantes*, so frequently seen both by Buffon* and Needham†, I should have suspected the goodness of my glasses, had I not compared them with several others, and found them among

* Buffon *Histoire Naturelle*.

† Needham *Observat. avec le Microscope*.

the best; and had I not also been always able easily to discover the animalcula in every one of those mixtures that were exposed to the air, even with the smallest magnifier I had.

For these reasons, though I am not willing to differ from so expert observers as Buffon and Needham, yet I cannot help thinking, that there must have been either some *deceptio visus*, or inaccuracy in the experiments themselves. If, for instance, the bottles made use of were not so exactly corked, as that no possible communication could exist between their contents and the atmosphere, either the ova of animalcula, which in the summer are every where floating about, might get into them and so be hatched, or, if these ova were previously contained in the ingredients before they were put into the bottles, they would in this case also be hatched, provided any intercourse was left between the contents of the bottles and the circulating air, either by their not being sufficiently

ficiently corked, or by small rents existing in them.

Needham, in order to obviate, as he thought, the possibility of animalcula or their ova existing in the liquors upon which he experimented, took the droppings of several sorts of roasted meat, in which he was sure that the fire had destroyed all the seeds of life, and infused them for some days in water, closely shut up in small phials, and yet found animalcula generated in all of them. But even this experiment is far from being conclusive; for it appears from his own words *, that he did not make use of these droppings till after they had turned to a jelly, which must necessarily have been in a place removed from the fire, and probably in the open air. Now, as I have observed above, it seems almost evident that in the summer the seeds of animalcula are every where floating about in

* Ayant donc pris de la gelée de veau & d'autres viandes grillées & roties. Need. *Observat. avec le Microscope*,

the air, and continually deposited upon all things around us, though they are never hatched but when they happen to alight upon a proper nidus: it is therefore highly probable that these jellies, before they were shut up by Needham, had received a number of the ova of animalcula in this manner, during the time that they were cooling; which ova might, by an extremely small communication with the circulating air, be afterwards hatched into animalcula; or, if the phials into which these jellies were put, were not carefully and immediately washed with boiling water, before the liquors intended to be experimented upon were put into them, then the bottles themselves would contain the seeds of animalcula. But, even allowing any of these to have been the case, some small intercourse with the external air appears, from several of the above experiments, to have been essentially necessary, not only to the receiving at first, but also

to the continuation of the living principle.

It may be objected here, that the animalcula in *semine masculino & feminino* *, seem not to be generated from the ova of animalcula taken in with our food, and are also so closely shut up in the *vesiculæ seminales*, that they can hardly receive any air, and yet their existence has never been denied. Whether the original seeds of these animalcula enter into our bodies from without, or are generated within us, is, what we are as yet, and perhaps ever will remain ignorant of. But it appears from experiments, that there is a considerable quantity of elastic air in every part of a living animal †; and it does not seem inconsistent with reason, that the animal fluids, in which animalcula are generated, should communicate some of their air to these animalcula as a kind of pa-

* Both Buffon and Needham suppose a female as well as a male semen, because they found the same animalcula existing in each of them.

† Hale's *Stat. exper.*

bulum to their existence ; at least, I think we may suppose, that a circulating or moving fluid can communicate this air more effectually to its inhabitants, than a stagnating one, whose motion is almost entirely stopped by being shut up in a phial, and which, perhaps too, contains less of this elastic air in its composition.

Having by the preceding experiments fully proved, that animalcula cannot be generated, nor exist without some communication with the open air, I now went on, to make some trials, with a view to discover by what things those already existing were most easily destroyed. But, before I relate these trials, I think it necessary to observe, that any thing which killed the animalcula generated in one animal or vegetable infusion, killed those also generated in any other with the same ease ; and any thing which did not kill those generated in any one animal or vegetable infusion, never killed those generated in any other : so that it would appear, though these

these animalcula seem to be of different genera and species, that the same things are noxious or salutary to the whole of them*.

EXPERIMENT XXXIX.

I filled the concave glass of my microscope, which held about a tea-spoonful, with an infusion of several different plants, crowded with animalcula, and put into it one grain of the powder of Peruvian bark. As soon as the bark subsided, I saw the animalcula, very distinctly, running about as briskly as before; in about ten minutes their motion became more languid, and in less than a quarter of an hour they were all dead. I then put five grains of bark into two ounces of this infusion. Five minutes after I examined some of it, and found the animalcula moving about slowly: twenty minutes

* The author does not mean here to affirm, that any thing which does not kill the animalcula generated in one animal or vegetable substance, cannot possibly kill those generated in another, but only that it happened so in all the trials he made.

after,

after, they were all dead. This liquor was then set in the open air, and, after having stood about three weeks, was again examined, and a numerous brood of animalcula again found in it, though not quite so many as before.

EXPERIMENT XL.

The concave glass of the microscope was again filled with an infusion of the same plants: an almost imperceptible bit of corrosive sublimate was dropped into it; but, though the focus of the glass had previously been adjusted to the animalcula, before I could bring my eye to a proper distance, so as to see through it, they were all dead.

Calomel was tried next, and generally killed the animalcula in about seven or eight minutes; but crude mercury never had the smallest effect upon them.

EXPERIMENT XLI.

Camphor, being tried several times, always killed them in less than five minutes, though an extremely small portion of it can only be dissolved in any watery menstruum.

EXPERIMENT XLII.

Common sea salt, in the quantity of a grain, to the full of my concave glass of the infusion, killed them in less than two minutes; if the proportion of the salt was greater, it killed them almost instantaneously*. All the other saline substances which I tried, destroyed them sooner or later, according to the different strength of these substances.

EXPERIMENT XLIII.

Sulphur killed them, but slowly, and sometimes not in less than a quarter of an hour. This I suspected was

* Needham also found that sea salt destroyed animal-
cula,

owing

owing to its being almost indissoluble in cold water, while in its pure and unmixed state; and I was confirmed in this opinion, by making trial of a solution of hepar sulphuris, which generally killed them in two or three minutes.

EXPERIMENT XLIV.

A few drops of oil being put into a phial of water, which contained a number of animalcula, and the phial agitated a little, so as to mix the oil and water imperfectly together; on examining it the animalcula were all dead. A small phial being also filled nearly full of water, which was crowded with animalcula, and a little oil poured on the top of this water; after it had stood a night, the animalcula were all dead. In this case, the oil appears to have operated by excluding the circulating air, in the same manner as a cork would have done.

EXPERIMENT XLV.

Solutions of Irish and Castile soap were next tried. All of them destroyed the animalcula in times proportioned to their different strengths.

EXPERIMENT XLVI.

Of all the different things which I tried for the destruction of these animalcula, lime-water seemed to have the most instantaneous effect. One single drop of it let fall, by the help of an assistant, into the concave glass, while I beheld them moving briskly about, transfixing them in a moment, and made them fall motionless to the bottom; and this always happened as often as I tried the experiment.

EXPERIMENT XLVII.

I put about a scruple of Rappee snuff into a small gallypot, along with an infusion which was crowded with animalcula: after it had stood a night, it was examined, and the animalcula found as brisk and lively as before.

From this time, it was examined every day, for about a week; but no change could be observed to have happened to them. A drachm of tobacco was then cut down, and put into another infusion which was full of animalcula; in this mixture also they lived, and seemed brisk till it was almost wholly evaporated.

Tobacco has generally been supposed to destroy all insects; it would therefore seem that animalcula are not of the same nature with insects, as they can live in it with impunity.

There are, doubtless, a variety of other things, besides these I have mentioned, which would destroy the animalcula bred in putrid infusions; but as the destroying them in this manner can hardly lead to any useful inference in the healing art, I shall not prosecute it any further; nor should I have said so much concerning it, had not curiosity drawn me on from one trial to another, till I had made all those I have now related.

I should

I should now proceed to draw a few of the inferences which more obviously result from an examination of the above experiments. But, before I enter upon that subject, it will, I hope, not be deemed foreign to my purpose, to make a few observations upon some of the arguments and proofs employed by those gentlemen who make animalcula the cause of putrefaction.

Here I should naturally begin with Kircher, the founder of this doctrine; but, as his arguments and proofs, though ingenious at the time they appeared in the world, have most of them been already exploded in these latter times, when we have become more intimately acquainted with natural history, and have a system of philosophy better established by experiments. I shall only mention a few of them; and am persuaded, that to mention them, will be fully sufficient to shew that they are not of consequence

quence enough to deserve any serious examination.

His first reason, why he thinks animalcula are the cause of putrid diseases, is, because he had seen not only all kinds of insects, but likewise various monsters of the venomous kind, generated in the interior parts of the earth: 2d, Because water inclosed in a vessel, and exposed to the sun, was found to have bred animalcula; and because worms are generated in the human stomach and bowels: 3d, Because, according to him, there is no species of plant which does not generate a worm peculiar to itself, when it becomes mouldy: 4th, Because the dead carcases of animals have a wonderful power of producing worms*: 5to, *Quia aqua per vaporem elevata, aer, grando, nix, vermibus scatent*: 6to, *Quia nullum vivens ali potest nisi ex iis rebus, quæ quandoque*

* Here I beg leave to observe, that this happens only in certain degrees of heat and moisture, and when such substances have some communication with the atmosphere, as appears by the 27th experiment, and some
K other

quandoque vita imbutæ fuerint; ita nullum quoque corrumpi potest, nisi ex iis rebus quæ quandoque vitam habuerint.

I shall now just mention one or two of his experiments, by which the reader will see, how a person, fond of an hypothesis, will often torture facts, in order to make them quadrate with it. In his first experiment, he says, that if you leave a piece of flesh, exposed for a night to the moisture of the moon, and examine it with a microscope next morning, you will find it to have degenerated, by the influence of that planet, into an innumerable quantity of putrid worms.

In his second experiment, he says, if a serpent be cut into small parts, put into rain water, exposed for some days to the sun, and then buried 24

other trials of a similar nature which I have since made, one of which is as follows: I buried a pound of fresh beef in a pot of earth, which I crammed as hard around it as I could; when it had lain three weeks, I took it out, and found its texture almost totally dissolved, its smell extremely offensive, but could discover no animalcula upon it, though it was in the middle of summer.

hours

hours in the earth, a great number of little serpents will be discovered in it by the microscope. I must beg leave here to observe, that this experiment proves no more, than that every animal substance, macerated in water, produces animalcula when exposed to the air; but that those have any resemblance to that animal from whose flesh they were generated, seems rather fabulous; though not long ago it was an opinion, that not only animal infusions produced animals like those whose flesh was infused, but that vegetable infusions also often produced salts which shot into chrystals resembling the plants from which these salts were obtained*. Several animals indeed have a power of reproducing certain parts of their bodies when taken away, and, being divided into various parts, they have a power also of producing intire and perfect animals from these sections of their bodies; but, in such cases, the life of

* *Vid.* Boyle.

none of these sections must be destroyed *.

The rest of his experiments I shall not take the trouble to examine: they are all nearly of the same nature with those I have already related, and tend to prove nothing more, than that animalcula are frequently generated in almost all putrid substances; which is a fact, although well established, yet widely different from their being the cause of putrefaction; for, whatever is the cause of any effect, must necessarily and indispensibly exist before that effect can be produced. Now I have shewn above, that putrefaction can exist, with all its usual phenomena and effects, without animalcula; therefore they cannot reasonably be supposed to be the cause of it.

The celebrated Linnaeus, who, by his ingenuity and labour, has not only methodised natural history, but also enriched it with observations, has likewise favoured the doctrine of ani-

* Spallenzani on animal reproductions.

malcula being the cause of putrid diseases. It is with deference that I oppose my opinion to that of a person so deservedly famous over all the learned world; which indeed I should not have done, had I not been doubtful, whether all the arguments and facts which he has advanced in favour of his hypothesis are sufficient to put it beyond doubt. Although animalcula be discovered lurking near the sides of the pustules in the itch; although the itch be generally cured by such things as are destructive to animalcula; does it from thence naturally follow, that animalcula are the cause of the itch?

The following reasons, I presume, make it extremely doubtful. First, Oil, so far as I have hitherto tried it myself, or can depend on the testimony of others, kills all kinds of animalcula; but oil rather seems to exasperate than alleviate the symptoms of the itch, which it ought not to do by the hypothesis, seeing it is noxious to animalcula.

Second, Solutions of Irish or Castile soap destroy all kinds of animalcula, either bred in putrid fluids or solids: these should therefore be a cure for the itch; but experience teaches that they have no such power.

Third, As all the exanthematous diseases are supposed by Linnaeus to arise from animalcula, and as musk is reckoned by him an effectual preservative against these diseases, it should therefore follow, that musk should be destructive to these animalcula, or, at least, hinder their generation; but it neither does the one nor the other: for I mixed musk, in various quantities, with several fluids which contained animalcula, but never could observe that it destroyed any of them; and small quantities of it put into water always generated an amazing number of them in a few days.

This reasoning will extend equally to all the putrid, exanthematous, and other cutaneous disorders. For, if they all depend on the same cause, viz.
ani-

animalcula, they must all necessarily yield to the same method of cure: and therefore, in such cases, whatever kills animalcula must be a specific, such as mercury*, lime-water, sulphur, oil, soap, and a variety of other things; but this does not happen in practice; therefore the hypothesis cannot be right.

But further: though Linnaeus supposes this hypothesis to be the most probable of all those that have hitherto appeared, yet he does not say, that he himself has seen the animalcula which are the cause of, and exist in putrid exanthematous eruptions†: and indeed, if we were most indubitably certain, that the gentlemen, whom he has quoted as the discoverers of them, had made their observations without any possibility of being deceived, which I am far from thinking to have been the case; yet, if they

* See the foregoing experiments.

† Vidit illa (scil. animalcula) in morbillis Langius; in peste Kircherus; in syphilitide (limacum similia) Hauptmannus; in petechiis Siglerius; in variolis Lulitanus & Procellus. *Amœnitat. Academ.* tom. v. p. 94.

were only seen in a few extraordinary cases, these will make but little towards ascertaining the hypothesis; for, before we can fix upon any thing as the efficient cause of an effect, it must not be occasionally, but constantly present whenever that effect is produced. But I think I am pretty certain, that, although animalcula may sometimes be found in exanthematous eruptions, and dysenteric stools, they are far from being common in either; and therefore, all that can be fairly inferred from their presence, is, that they are sometimes an accidental symptom in these disorders.

In looking over the history of diseases, one will find a great number of extraordinary and uncommon symptoms, which have now and then attended them. But I know no attempt that has hitherto been made, in order to persuade mankind that the causes which have once produced these symptoms must always necessarily exist in every disease where they have once been observed. It would therefore

fore seem, that the same identical cause, which, on some extraordinary occasions, has produced worms in dysenteric stools, is by no means always necessary to produce the dysentery itself, as it can evidently exist either with or without this symptom.

I shall not pretend here to consider every thing particularly which Linnaeus has alledged in support of his hypothesis. All that I have further to observe is, that he seems to think the long preservation of contagion in skins, linen, and other cloaths, and its being in this manner transported from the most distant regions, may be better explained by supposing this contagion to be the ova of animalcula, than by considering it as any other kind of matter. That the ova of animalcula may be kept long fresh, and in a state fit for being hatched, not only seems possible, but is also consistent with the experience of several authors *: but I can hardly think that even this affords

* Mead on the plague. Marc. Anton. Plenciz. Baker's microscope made easy.

any proof of their being the cause of contagion, as we have many other things in nature which will also keep long, and still continue in such a state that they will act almost in the same manner as when recent. Of this kind is yeast, and many more which might be mentioned. But I shall finish what I have further to observe concerning the arguments which have been used in support of the animalcular hypothesis, with a few strictures on some parts of the *Tractatus de Contagio* of Marc. Anton. Plenciz.

This author has carried his ideas of contagion, by means of animalcula, and their ova, much farther than any other that I know of; and has even gone the length of affirming, that every animal and vegetable substance, in every part of its composition, necessarily contains either animalcula or their ova; and that either of them, when they meet with a proper nidus and pabulum, are multiplied almost *ad infinitum*, and so produce putrefactive exanthematous disorders.

As

As a corroborating proof of this assertion, he cites the authority of Buffon, who I suppose was led into that opinion by some of his experiments, which I have already mentioned; and also an experiment of Lancisis, where he says, that animalcula were plentifully generated in a phial that was filled with nut water, and hermetically sealed. I have already observed, that I doubted some inaccuracy had taken place in the experiments of Buffon, and must do the same with regard to this of Lancisis, as I am pretty well convinced, from repeated trials of the same nature, that no animalcula are generated in a phial that is well corked, whether it contain nut water, or any other mixture; and far less will they be generated in one that is hermetically sealed.

He thinks he is certain also, that animalcula exist in all kinds of pus excreted from the human body, though he does not say that he has examined this matter, and seen them there himself.

self. But I cannot help doubting of the truth of this likewise, as I have repeatedly examined pus from various sores, and seldom found animalcula in it; but, when I did find them, they were always large enough to be visible by one of the middling sort of magnifiers; so that I can hardly doubt, if they had existed in every quantity of pus which I examined, but I must have detected them either by one of these glasses, or by the finer ones.

He allows, that animalcula may exist without putrefaction; but denies that putrefaction can exist without animalcula *. This last assertion is, however, proved to be erroneous, by several of the preceding experiments; and this proof is one of the strongest arguments which can possibly be brought against the animalcular hypothesis.

His method of accounting for the smell of putrid substances is, by sup-

* Sed hoc adversariis nostris concedere non possumus, quod putredo absque tali materia animata esse possit. *Traët. de Contagio.*

posing it to arise from the excrement of animalcula *. This fœtid smell, he says, takes place in all putrid bodies, except wood. But wood, by his own confession, as well as by the testimony of several others, is fully as much crouded with animalcula, when putrid, as any other substance †. Therefore, by the hypothesis, putrid wood ought to smell as fœtid as any other putrid body; but it does not; therefore the hypothesis must be wrong, or the wood animalcula must void no excrement, or their excrement must have no smell. That they should void none, is hardly consistent with the laws of animal life, so far as we are acquainted with them; that it should not smell fœtid, if that of other animals in putrid substances smell so, is hardly to be credited or accounted for.

Our author seems much at a loss how to extricate himself when he

* Et quod ab his experimentis, fluida quæ putrescunt, turbida fœtida reddantur. Idem *Tract. de Contagio*.

† Lewenh. in epist. ii. ad Domin. Herman. Van Zoële. Ray's *Wisdom of God in the Creation*.

comes

comes to consider the objection by which it is alledged, that animalcula are the effect, and not the cause of putrefaction. *Ast qui pertinaciter contrarie (inquit) adherent sententiæ, aliud in his jam adductis argumentis non inveniunt effugium quam quod dicant, vermiculos illos, qui in rebus putridis observantur, esse effectum putredinis, non vero ipsam putredinem, adeoque putredo debeat esse aliquid aliud, & ab alio principio dependere. Verum quale illud principium sit, in quo vero putredo consistat, non determinatur, neque determinari possit; nam superius jam ostendimus, in illis quæ adversarii asserunt, veram naturam putredinis constitui non posse; igitur nihil superest nisi materia quædam animata, per quam putredo constituitur.* This passage affords a view of an argument of the strangest nature I ever met with. To say, that because putrefaction, or because any one thing you please, does not consist of this, or of that, there is therefore nothing else in the creation left for it to consist of, is by no means talking like a philosopher; who, if he has made any proficiency in that science,

science, must be conscious how small a portion of the works of the great author of nature fall within the limited span of his knowledge.

Upon the whole, this author seems to have been a little too fond of his hypothesis ; and, in order to prove it, has laid hold of, and adapted to his purpose, every thing he could meet with, that had the smallest relation to the existence of animalcula in putrid substances, but has not advanced one single experiment of his own ; relying wholly on the observations and relations of others, with regard to which he has been abundantly credulous.

Having now, I flatter myself, shewn the insufficiency of some of the leading arguments made use of by those gentlemen who favour the opinion of animalcula being the cause of putrefaction, I shall proceed to draw a few more conclusions from the above experiments, and to give a summary view of what has already been said in this chapter.

By

By the 22d and 25th experiments it appears plain, that putrefaction can take place, either in an animal or vegetable body, without producing any animalcula *. Whence it follows, that if, in any single case or circumstance, it can exist without them, they are not the efficient cause of it ; but they are so far from being the efficient cause of putrefaction, that a piece of silk, or cloth †, tied over the putrefying substance, is only required to keep them from being generated in it.

By the 27th experiment it appears, that though the ova of animalcula exist in any solid or fluid, yet they cannot be hatched without the assistance of the external air ; and that even a pretty large quantity of inclosed air is insufficient for this purpose, farther appears, from subsequent trials, as mentioned in the same experiment.

It would seem, however, that an exceeding small quantity of fresh cir-

* Marc. Anton. Plenciz. in his *Tractatus de Contagio*, expressly denies this.

† *Speclacle de la Nature*, tom. i. p. 15.

culating

culating air is sufficient to allow a vivifying principle to be communicated to these ova; for animalcula were plentifully generated in a small quantity of Leontodon and water, in the same large bottle with which I had experimented before, when, instead of being closely corked, a piece of tobacco-pipe-stopper was introduced through the middle of the cork, so as to open a small communication with the external air: from which I think we may safely infer, that wherever animalcula exist, whether in solids or fluids, their existence is a proof that there is circulating air in the place where they are found.

Nothing can be more natural than to conclude, that, if animalcula are the cause of putrefaction, this process will be accelerated by bringing a number of them into contact with any putrescible substance; but by several of the preceding experiments the direct contrary happened; therefore they have no such power.

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But further: if they are the cause of putrefaction, as the cause and the effect must necessarily co-exist together; therefore we must conclude, that wherever putrefaction exists, animalcula must exist also. This we have already found not to be the case; but it will follow from this proposition, that, wherever animalcula exist, putrefaction must exist also. This too does not happen; for we have found, by the 29th experiment, that they were generated as plentifully where no signs of putrefaction could be discovered, as where it took place in the most obvious manner.

Nor should animalcula only be co-existent with putrefaction, but previous to it, if they are the cause of it: which however seems not to be the case; for, after the most careful examination, I have always found the putrid smell at least several hours before I could observe any animalcula, unless where they existed in the ingredients previous to their running into a putrid state.

We

We have already seen that putrid vegetable matter strongly preserves animal substances from putrefaction. By the 28th experiment we have also seen that putrid vegetables, when they have finished their fermentative process, do not hasten the putrefaction of fresh vegetables. Nevertheless it would seem, by the 32d experiment, that the maggots generated in putrid animal matter hastened the putrefaction of fresh animal matter. But, though the maggots then made use of were carefully washed, they might still retain something of the putrid colluvies from which they were taken; or possibly those of them which seemed dead might have run into the putrid state, and been the occasion of the smell which was supposed to have arisen from the mutton. But, should we even allow that these maggots were the cause of this piece of mutton becoming putrid sooner than the other, even then it is not a sufficient proof that animalcula are the cause of putrefaction, as the mag-

gots made use of were a much larger species of creatures, and of a nature different from those almost invisible ones upon which putrefaction is said to depend by the gentlemen who favour this hypothesis.

The greatest part of authors who have wrote concerning the animalcula generated in putrid animal and vegetable matter, seem always to have considered them as necessarily existing there, independent of circumstances. Were this really the case, it would be a strong argument in favour of the animalcular hypothesis; but, as we have already seen, that, besides the animal or vegetable matter itself, the circumstances of heat, moisture, and season are also necessary towards their generation, their existence proves nothing. But, lest what I mean by season being necessary towards the generation of animalcula should not be understood, I shall explain it by repeating an observation already mentioned, viz. that, at any of those degrees of natural heat which we generally

generally have during the summer, animalcula will be plentifully generated from animal or vegetable matter during that season; but in the winter, let any of these degrees of heat be artificially raised, let animal or vegetable matter be placed in them, and no animalcula will be generated*.

And indeed, from all the trials I have hitherto made, no animalcula have been generated during this month (December 1768) in the same mixtures in which they were plentifully generated last summer, though I have tried every degree of heat from 50 to 80 of Fahrenheit's scale: hence it appears, that animalcula are only an attendant on putrefaction for one part of the year in our northern climate; and

* Infusions of pepper, of Leontodon, and of animal matter, from all of which I never failed to obtain animalcula last summer, being repeatedly tried this winter, in various degrees of heat, never once produced any. Since these experiments were made, I have been told by a gentleman, that he has produced them in hay in the winter; and Mr. Ellis, in the Philosophical Transactions for the year 1770, says, that he produced them from infusions of potatoes. As I made my experiments with all the accuracy I could, the matter with me is still doubtful.

therefore cannot be the cause of it, as it attacks the living subject indiscriminately at all times of the year, and the dead one also, though in the winter it comes on more slowly than in the summer.

C H A P.

C H A P. IX.

Some attempts to discover whether the loss of the fixed air contained in bodies, be the cause of their decomposition and putrefaction.

THE illustrious Sir Isaac Newton was the first who, among many other important discoveries, demonstrated, that fixed air was an ingredient in the composition of almost all bodies.

But the hints given by Sir Isaac on this subject seem to have been but little attended to, till the learned Dr. Hales further elucidated them, not only by shewing that fixed air was a part of the composition of almost every animal, vegetable, and mineral, but that it could likewise be obtained from them in various manners; that, with regard to gravity and elasticity, it had some affinity to the common air of the atmosphere, but differed from

it in being totally unfit for respiration *.

From the hints of these great men, Dr. Macbride, in his late ingenious essays, has endeavoured to illustrate the nature of this fluid still farther, by a variety of curious and judicious experiments; by which he endeavours to prove, that it is the cementing principle, or bond of union, which connects the *ultima corpuscula* of bodies to one another, and preserves them from running into a state of decomposition and putrefaction, to which all animal and vegetable substances have a natural tendency, and to which, according to him, they arrive with more ease when divested of this principle.

The experiments and arguments made use of by this gentleman in support of his hypothesis seem so conclusive, that, so far as they regard fixed air, I should have rested on their

• *Vegetable Statics,*

validity,

validity, had not some experiments that I had made on the same subject, previous to the reading of his book, induced me to think the matter at least doubtful. After I had read his book, I became doubtful of my former experiments, therefore determined to repeat some of them, and add a few more, for my own satisfaction, on a subject that still appeared dark to me. These I now lay before the public for their candid examination, without having drawn any other inferences than such as I think naturally result from them; nothing being farther from my intention than to cavil, nor any thing more disagreeable than to canvass the sentiments of other people, into which nothing could possibly have led me but the connection of that plan which I am obliged to prosecute.

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EXPERIMENT XLVIII.

On the 24th of June 1765, having put a small Seville orange into a sheep's bladder, and squeezed out as much of the air as I could, so as to bring the bladder into contact with the orange all around its surface, I tied the mouth of it close to the orange, and secured the section with sealing wax. A small turnip was put into another bladder, and sealed up in the same manner. The bladders, with their contents, were then laid together in a warm closet. The orange in about three weeks was contracted to nearly one half of its original size, and had emitted nearly about as much fixed air as it had lost in magnitude. The turnip contracted so fast, that in about six or seven days it was reduced nearly to one-fifth part of its former bulk; but no air had blown up any part of the bladder around it. The orange, when taken
out

out of the bladder, though reduced to a kind of pulp, had acquired no offensive smell; the turnip was become almost as hard as a piece of horn, and had no smell at all.

Though no visible hole could be discovered in any of the bladders, yet I suspected the tightness of that one which contained the turnip: In order therefore to put it to the trial, I poured into it three ounces of water, tied and sealed it as before, and having laid it in the same closet, when it had remained there eight days, the bladder and water together only weighed three drachms. I then poured the water out, blew up the bladder, sealed it and laid it again in the same closet; after eight days more it did not seem to have lost any of this air, nor could I force any of it through the bladder, though I squeezed so hard that I was afraid of bursting it.

EXPERIMENT XLIX.

Having resolved to repeat this experiment, I put a larger turnip, of five inches diameter, into a strong ox bladder, which I tied and sealed as before. It was then laid in a moist warm place, where it remained several days without parting with any fixed air: being afterwards removed into a dry place, and kept there about a month, the turnip was found shrivelled up nearly to the size of a walnut, and the bladder adhering closely every where to its surface. On opening the bladder, the turnip had acquired nearly the consistence of horn, but had no smell.

EXPERIMENT L.

A small apple was next inclosed in a sheep's bladder, which was laid in the same closet. After it had lain there about a month, the apple was become
soft,

soft, and had emitted a little fixed air: when it had lain six weeks, the whole was put into a basin of warm water; which soon rarifying this air, made it burst the bladder. When a part of the apple came out, I at first thought that it smelled putrid; but, on a more narrow examination, it appeared that this smell arose from the bladder, which had been macerated in the warm water.

An experiment of this kind was also attempted by inclosing a piece of beef in a bladder; but in this manner it did not succeed; for these two animal substances putrifying nearly about the same time, the structure of the bladder was thereby destroyed; so that it, together with the beef, fell down into one putrid mass.

The intention of these experiments was, to discover whether fixed air could be separated from bodies without these bodies running into a putrid state, which I conjectured might possibly happen, and which was justified by the event; for, though the texture both of
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the orange and the apple was broke down and destroyed, neither of them were putrid.

The texture of the turnips was not destroyed: I therefore suspected that the fixed air contained in them, in their expanded state, had also been shrivelled up and condensed along with them; and accordingly, upon trial, I obtained nearly the same quantity of fixed air by distillation from that shrivelled one which had formerly been five inches in diameter; as I could from a fresh one of the same dimensions; from which it would seem, that the fixed air of the turnips had not exhaled along with their aqueous parts, as I imagined, but had remained still with them, and possibly preserved them from being dissolved: For I agree with Dr. Macbride in thinking, that, whenever the fixed air is extricated from any solid body, that body must either necessarily be dissolved, or suffer some change in the internal structure of its parts; though I am far from being of opinion

nion that this change must induce putrefaction: for, if the vinculum of bodies, or, in other words, that which preserves them from putrefaction, be fixed air, then it will naturally follow, that those bodies which contain the greatest quantities of this fixed air, should not only be the longest of turning putrid themselves, if they retain this vinculum, but likewise the strongest preservatives against the putrefaction of other bodies when they part with it, provided these other bodies have the power of absorbing fixed air.

The experience of people who have made long sea-voyages, as well as that of the best medical authors, has long established this fact, that lemons, oranges, and other succulent vegetables, are amongst the most powerful antidotes against that species of putrefaction called the sea-scurvy; on the principles therefore of Dr. Macbride, they ought to contain greater quantities of fixed air than other vegetable substances, and

part with it more easily in the stomach, in order to supply it to the animal, which, in these circumstances, stands in need of it.

EXPERIMENT LI.

With a view to throw some light upon this matter, I took an orange and a turnip of equal weight, and distilled each of them in a small apparatus for bringing over fixed air: by this process I obtained rather more air from the turnip than from the orange; therefore, by the hypothesis, turnips should be a stronger antiseptic than oranges.

The same thing was tried with a lemon, and a turnip of equal weight. The fixed air obtained from the turnip was nearly the same as before; but that obtained from the lemon was less than what had come from the orange.

EXPERIMENT LII.

With a view to discover whether lemons and oranges would part with their fixed air in the stomach easier than other vegetables, I put into one eight ounce phial an ounce and a half of bread, half an ounce of the juice of lemons, and half an ounce of saliva. Into another phial of the same size, one ounce and a half of bread, half an ounce of the juice of oranges, and the same quantity of saliva. Into a third, an ounce and a half of bread, and half an ounce of saliva. The phials being thus prepared, were all filled up with water. Bladders were fastened to the mouths of each of them, and they were placed all together in a heat as near to that of the human stomach as could be kept up. The two phials which contained the acid began to be in motion in a few minutes, and soon after to throw up some of their solid parts to the top; and in half an hour from the time

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that they were set down, they had completed the throwing up of all their solid parts, and an exceeding small quantity of air had come over into the bladders from each of them. After the standard phial with the bread, water, and saliva had stood about ten minutes, the motion in it was also become perceptible: when it had finished its fermentation, which it did before the other two, a quantity of air of the size of a small walnut had come over into the bladder. The two acid mixtures never fermented briskly. When they had finished that process, a quantity of air, nearly of the magnitude of a filbert nut, had come over from that one with the orange-juice, and about half as much from the other.

Here the circumstances were as similar to those of digestion in the stomach as I could make them; but the lemon and orange-juice, instead of facilitating the extrication of the fixed air, rather hindered it: should the same thing take place in the stomach,

as

as we have reason to believe it will, then lemons and oranges are not antiseptic by the hypothesis, but they are antiseptic by experience; and every hypothesis that does not quadrate with facts that are established by experience must be faulty.

EXPERIMENT LIII.

I took five eight ounce phials, and marked them 1, 2, 3, 4, 5. Number 1, was filled with wort; number 2, was the acid mixture, with the juice of lemons, as in the last experiment; number 3, was that with the orange-juice; number 4, contained half an ounce of exceeding salt beef, made into a mash with pure water, and half an ounce of saliva, and was filled up with water; number 5, contained half an ounce of boiled cabbage leaves, and as much saliva, and was likewise filled up with water. Bladders were fastened to the mouths of all the phials as before, and they were set in the same degree of heat. The fer-

mentation began soonest in number 1, and continued about two hours; at the end of which, a quantity of air, nearly an inch in diameter, had come over into the bladder. Number 2, and 3, began their fermentation soon after number 1, and finished it some time before it: but in this experiment the greatest quantity of air came over from the mixture with the lemon juice; whereas in the last, that with the orange juice had yielded the greatest; the quantity from each, however, seemed rather less than in the last trial. Number 4, began to ferment in about 20 minutes, and continued to do so for several hours. The quantity of air that came over from it was considerably larger than what had come from both the acid mixtures. Number 5, with the salt beef, was longer of beginning to ferment than any of the others, and continued also longer in that state. The quantity of air that came over from it was about the same as had come from the wort.

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The phenomena arising from some of these mixtures, seem to favour the theory of Dr. Macbride; and those arising from the others, seem to invalidate it, particularly the air arising from the salt beef: for, if the cementing principle of bodies be fixed air, and this fixed air be the principle by which they become antiseptic; and if salt beef contain a greater quantity of it than lemons and oranges, or at least part with a greater quantity of it during fermentation, then it must necessarily follow, that salt beef is a stronger antiseptic; and if so, why do scurvies attack sailors with so much violence, when this antiseptic beef is a principal part of their food?

But further: if salt beef contain more fixed air than lemons and oranges, part with it easier, and be thereby a stronger antiseptic, whence arises the absurdity, that sailors so often contract putrid scurvies while they are constantly using the stronger antiseptic, salt beef; and are after-

ward certainly cured of it by using the weaker one, lemons or oranges ?

EXPERIMENT LIV.

Dr. Macbride is of opinion, that the antiseptic power of the bark, is owing to the fixed air extricated from it while fermenting in the stomach. On reading this, I suspected that there might be many other vegetable substances which would yield as much fixed air in fermenting as the bark, and yet be greatly inferior to it in antiseptic power. In order to try this, I took six eight ounce phials, and having marked them 1, 2, 3, 4, 5, 6. Into number 1, I put half an ounce of powdered bark, and the same quantity of bread and saliva: into number 2, I put half an ounce of sliced turnip, and as much bread and saliva: into number 3, I put half an ounce of cabbage leaves, and the same quantity of bread and saliva: into number 4, I put half an ounce of sliced carrot, and the same quantity of bread

bread and faliva : into number 5, I put half an ounce of dried peas, and the same quantity of bread and faliva : and, into number 6, half an ounce of potatoe, and as much bread and faliva. Bladders were now fastened to the mouths of all these phials, after filling them up with water, and they were placed as before, in a heat near to that of the human stomach. It would be needless to relate the various times at which they began their fermentation, and different spaces which they continued in it ; these are foreign to our purpose : suffice it therefore to say, that after the whole of them had finished this process, number 1, 2, and 3, had all of them yielded nearly equal quantities of air, viz. about the bigness of a large walnut ; number 4, had yielded something less ; number 5, had yielded nearly twice as much as the bark ; and number 6, had given considerably less than any of them.

Bladders that are used in this manner, for the reception of fixed air,

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must

must be previously steeped in oil ; but even then they are sometimes apt to contract so with the heat, during the experiment, as to render it somewhat uncertain. As much, in my opinion, depended on the certainty of this last experiment, I resolved to repeat it again in a manner which would enable me to measure the quantity of air extricated from each mixture : this was by a bended tube and a phial, in a vessel of water, suspended upon the end of it, as described by the honourable Mr. Cavendish, in the Philosophical Transactions for the year 1756 *.

The six mixtures, as in the last experiment, were tried with this apparatus ; and from number 1, I obtained by fermentation, seven drachms, by measure, of fixed air.

From number 2, I obtained rather more than seven drachms.

From number 3, eight drachms and a half.

* Philosoph. Transact. for the year 1756, Table 7.

From number 4, only a little more than half an ounce.

From number 5, two ounces one drachm.

From number 6, I got little more than three drachms.

From half an ounce of apples, and the same quantity of bread and saliva, put into eight ounces of water, I obtained fourteen drachms of fixed air †.

The experiments on this subject, taken together, seem plainly to prove, that several of those vegetable substances, which are daily used at our tables, contain, or at least part with, more fixed air, during fermentation, than the bark. If, then, throwing fixed air into the blood be the natural method of preserving an animal from putrefaction, and likewise the method of restoring it to soundness, when already putrid, pease, apples, cabbages, turnips, ought all to answer this intention more powerfully than the

† The air being infused into lime-water precipitated the lime.

bark:

bark: But, if any of them had such a power, it is reasonable to presume that their constant use must have discovered it, even though mankind had been less attentive than they are.

That bark, when taken into the stomach, has a power of stopping partial mortifications, is a fact already well established †; but, though the thing has not been much taken notice of, it further also possesses a power of contributing greatly to check these mortifications, when applied to them either alone, or along with other ingredients, in form of a poultice. Should we allow, that, when taken into the stomach, it checks them solely by throwing fixed air into the blood, yet we will find, that it has not so good a chance to throw this air into the blood, when applied as a poultice; for it can neither ferment so properly in this form, as when inclosed in the stomach; nor, if it could, the fixed air extricated from it would

† Medical Essays, vol. i.

not find so easy a passage into the blood; the extremities of the inhalent vessels in mortifying parts being always covered over either with a kind of eschar, or a quantity of inert matter.

But, should we allow that the bark, even when applied as a poultice, ferments, throws out its fixed air into the gangrened part, and thereby saves it from a total mortification, this would hint to us a process, by which the same thing might be accomplished much more fully and speedily, viz. by throwing this fixed air into it from a fermenting or effervescent mixture. I know not that this has ever been tried; it is, however, a safe, curious, and easy experiment, and, as such, I would recommend it to surgeons of hospitals, where such kind of gangrenes most frequently occur.

From what has already been observed, I think we have reason to suspect, that there are other principles in bodies, besides their fixed
air,

air, which contribute perhaps more than the fixed air itself to render them antiseptic. This supposition seems to be much supported by some of the experiments of Sir John Pringle, and particularly by that one where two grains of camphor proved a much stronger antiseptic than sixty grains of sea-salt. Now, let us suppose these two grains of camphor to have contained any quantity of fixed air, that can possibly be conceived as residing in so small a body; let it likewise have quitted the camphor, and been absorbed by the meat; yet it will still appear too diminutive to have been the sole agent by which this meat was preserved. But camphor, as has been proved by Dr. Hales, contains almost no fixed air; and therefore it certainly did not act by throwing it into the body which it preserved*.

Should we suppose the antiseptic power of the camphor not to have been owing to the fixed air inherent

* Vide *Vegetable Statics*.

in, and communicated by it to the body which it preserved, but to some peculiar power that it has of hindering the separation and escape of fixed air from other bodies to which it is applied, this would, in some measure, explain its antiseptic power: But this is not the case; for it will appear from some experiments, just now to be mentioned, that fermenting mixtures, with small quantities of camphor, throw out equal quantities of fixed air, with the same mixtures which contain none.

EXPERIMENT LV.

Half an ounce of raw beef, the same quantity of bread and of saliva, with four grains of camphor, were put into eight ounces of water, and set to ferment as before. The fermentation was several hours later of beginning in this mixture, than in those of the same nature without camphor: when it had begun, it continued longer also in this state, during which, five ounces and seven drachms of
fixed

fixed air had come over from it. This fixed air had no smell of camphor. The mixture, during its fermentation, never acquired a bloody colour, which all the others, when made with raw meat, had done; and whereas the others generally became putrid in 24 hours after they had finished their fermentation, this was kept eight days after it, and found perfectly sweet.

EXPERIMENT LVI.

Another mixture, containing the same quantity of raw beef, bread, and saliva, with two drachms of nitre, was set to ferment in the same manner. It was long of beginning to ferment, like that with the camphor, continued about the same time in that process, and yielded not quite five ounces of fixed air; it never acquired the bloody colour; and was likewise kept eight days after the fermentation without becoming putrid.

From the two last experiments it appears, that though some, and perhaps all

all antiseptics have a power of retarding the fermentative process of alimentary mixtures, they have no power of hindering it altogether; nor does it seem that they have any power of hindering the separation of fixed air from bodies while they are fermenting; for as much air was extricated from the two last mixtures, when they had the camphor and nitre, as when they had none of them. This, perhaps, is a rule that will hold good with regard to all antiseptics whatever; therefore antiseptics, at least camphor and nitre, cannot be said to preserve bodies, by restraining their fixed air from flying off, but by some other principle, with which we are as yet unacquainted.

It may possibly be alledged, that the reason why these two last mixtures did not become putrid during the eight days that they were kept after they had finished this fermentation, was, because they then absorbed a fresh quantity of fixed air. Had this really been the case, why was

it not the case also with the other alimentary mixtures, as they had the same chance to have absorbed this fixed air, and the same necessity for it? But it would seem that neither those mixtures with the antiseptics, nor those without them, absorbed any fixed air; at least, if they did, it neither had a power of exciting a new fermentation in them, nor was extricable by distillation.

I flatter myself I have now proved that antiseptics do not preserve those bodies, to which they are applied, by communicating fixed air from the antiseptics themselves, to the bodies which they preserve, nor by restraining the escape of fixed air from them. I shall therefore go on to consider how far fixed air, when separated from one body, is an antiseptic when applied to another.

EXPERIMENT LVII.

Having obtained about four ounces of fixed air from fermenting wort,
about

about the same quantity from wort with a piece of fresh mutton in it, and about half the quantity from a mixture of bread, water, mutton, and saliva, I resolved to try what the effect of each would be upon a slice of meat which had just begun to putrify.

This air had come over into bladders, fixed upon the mouths of phials, which contained the fermenting mixtures. That I might introduce the putrid meat into it, without allowing it to escape, I squeezed it into the bottom of each bladder, and secured it there by a tight ligature: having then dried the bladders, I introduced into the mouth of each of them two drachms of mutton, a little putrid, squeezed all the air I could from about them, and then having tied the mouths of the bladders with another ligature, removed these I had made before, by which means the pieces of mutton in the bladders were now surrounded only with fixed air. They were left in this manner four hours, at the expiration of which, none of

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the fixed air seemed to be absorbed, nor was the mutton in the smallest degree sweetened.

The same experiment being repeated, the pieces of mutton were allowed to lie 24 hours in the fixed air, but even then they did not seem to be in the least sweetened.

EXPERIMENT LVIII.

I cut a thin slice of beef from a larger piece that had fairly begun to have the putrid smell, and brought over a stream of air upon it from an effervescent mixture of distilled vinegar and salt of wormwood; but by this the beef did not lose any thing of its putrid smell. I then suspended it in the neck of a wide-mouthed bottle, while four ounces of distilled vinegar was made into *spirit. Minderer.* with the same salt. When the effervescence was finished, the beef had acquired a smell something between that of putrefaction and the spirit. I then put the same piece into distilled vinegar,

vinegar, and added as much salt as neutralized it. By this process, the beef seemed to have intirely lost its putrid smell, and to have acquired that of the spirit in which it had been immersed. I suspected, however, that this change was owing to the smell of the spirit having only overpowered, though not destroyed, that of the putrefaction; I therefore washed the piece of beef in water, by which it lost intirely the smell of the spirit, and again recovered its putrid smell. I then immersed it again into the spirit, and, on taking it out, it had again lost its putrid smell, and acquired that of the spirit: on washing it again in water, it lost the smell of the spirit, and recovered its putrid one. This method of treating it, I repeated several times, and found that it always had precisely the same effect; so that the beef was not really sweetened, its putrid smell only being lost in that of the neutral spirit.

EXPERIMENT LIX.

A thin slice of beef, just become putrid, was put into a four ounce phial, which was afterwards filled up with water. Air was then brought over, from a bottle of brisk small beer, by the apparatus formerly described, into the phial, so as to force all the water out of it: it was then corked, and kept 24 hours; at the end of which time, the beef was found rather more putrid than before the experiment.

Another slice of the same beef was, at the same time, put into the neck of a bottle of brisk small beer, while it fermented before a fire; after half an hour, the beef was not in the least degree sweetened.

EXPERIMENT LX.

A thin slice of beef, just become perceptibly putrid, was put into an eight ounce phial, and the phial filled with air from an effervescent mixture
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of common vinegar and falt of hartshorn. After remaining one night in the phial, it did not seem in the least sweetened.

The same piece of beef, however, after lying one night in vinegar, strongly saturated with falt of hartshorn, had become perfectly sweet.

Fixed air was afterwards brought over from several other fermenting and effervescent mixtures, upon pieces of meat just beginning to putrify; some of which were rendered a little sweeter by the process, but none of them were ever so much sweetened by it, as to lose intirely their putrid smell.

EXPERIMENT LXI.

Having already proved, that several substances can part with their fixed air without becoming putrid, and that substances already become putrid, are but little altered from that state, when fixed air, arising from the most antiseptic bodies, is applied to

them, and nothing altered from it by the application of fixed air from substances that are not antiseptic, I resolved to try whether it was possible for a piece of meat to turn putrid without parting with any fixed air. For this purpose, I took about a drachm of putrid beef, and about an ounce of fresh beef, and minced them small together. This mixture I crammed as hard as I could into a small phial glass: when it was full, I corked and sealed it, having fastened a piece of packthread into the cork, by which I might pull it out. After the phial, thus prepared, had stood about a month in the air, in winter, I put it into a bowl of water, kept it down with one of my hands, and, with the other, pulled out the cork by means of the packthread. None of the beef issued out at the mouth of the phial; and only a few bubbles of air arose to the top of the water; which, I think, would not have been the case, if much fixed air had been separated from the beef, as it would then

then have rushed out of the phial where it was confined, with violence: the whole of the contents of the phial were now equally putrid.

EXPERIMENT LXII.

Some beef prepared, as in the last experiment, was screwed hard into a small ivory box. The box being kept about a month, was put into a bowl of water, and a small hole drilled through it with a steel instrument: only two or three bubbles of air issued from this hole to the surface of the water, which, I suppose, would have happened, if the box had been pierced immediately after the beef was put into it, as it was impossible to fill it so exactly with the beef, but some air must have lurked in it. As I could not measure the quantity of air which arose to the surface of the water in the two last experiments, it is impossible to say whether any of it was separated from the beef; but, if any was separated, it must have been

extremely little: and therefore I imagine, that when an exciting *fomes* of meat, already putrid, is applied to meat that is still fresh, the whole may be thereby changed into a putrid state, without undergoing that species of fermentation which would otherwise be necessary before it could possibly arrive at that state.

In order to finish the experiments upon fixed air, I now determined to try whether I could extricate as much of it from a body, by any other method more speedy than fermentation, and what would be the effects of such an extrication.

EXPERIMENT LXIII.

For this purpose, I prepared the mixture number i, experiment 54, and by boiling it about a quarter of an hour over a brisk fire, I brought over, by means of a long flexible tube, into another phial, nearly about the same quantity of air as had come from

from it by fermentation. The mixture number 2, experiment 54, was next tried: something less of the fixed air came over from it by the boiling, than had come over by the fermentation. Number 3, being tried, yielded considerably more than it had done by fermentation.

EXPERIMENT LXIV.

As the mixtures just now experimented upon, were compounds, I could not possibly determine from what part or parts of their composition the fixed air had been extracted: I therefore took two ounces of fresh beef, put it into eight ounces of water, in a phial, and fixed to the mouth of the phial, the apparatus for receiving the fixed air. This mixture hardly ever fermented, and did not part with above a drachm of fixed air: on opening the phial, it was, however, extremely putrid. The same mixture, by boiling, yielded near an ounce of fixed air, and was not putrid. This
expe-

experiment was repeated several times, and the effects were always nearly the same.

The common process of roasting and boiling meat, is a quicker method of extracting the fixed air from it than fermentation; but neither of these processes hasten putrefaction; nay, it is a fact universally known, that they retard it; for it is common to dress meat in the summer season to keep it from spoiling; and, even after it has begun to spoil, it will, by this method, keep several days longer than it would otherwise have done: this fact, therefore, does not correspond with the hypothesis which supposes the loss of fixed air to be one of the principal causes of putrefaction*.

The following experiment likewise appears a strong proof against the hypothesis:

* It is not all fixed air that comes from meat while roasting or boiling, as it takes more of it to cause a precipitation in lime-water, than of any other fixed air which I tried.

EXPERIMENT LXV.

A small turnip was put into an ox bladder; the mouth of the bladder was firmly tied, to prevent the air from escaping: being put into boiling water, in a few minutes the bladder was blown up all around the turnip, by a quantity of air which seemed nearly of an equal bulk with the turnip itself. The turnip being taken out of the bladder, was put into a bowl of water. Another raw turnip, nearly of the same magnitude, was put into another bowl of water, and they were both set together in a warm place: the contents of the two bowls, being examined from time to time, began, as nearly as I could observe, to turn putrid together; whereas, if the loss of fixed air operated so strongly in producing putrefaction, the turnip which was boiled, having thereby lost a considerable quantity of its fixed air, ought to have putrified long before the other.

Dr.

Dr. Macbride mentions boiling among the methods which he has enumerated for preserving substances from putrefaction; and, speaking of meat, accounts for its having that effect, by thickening the external coat of it, so as to prevent the escape of the fixed air: But it appears by the experiments now recited, that meat, and other substances, lose more of their fixed air by a few minutes boiling, than they would do by several days keeping; and therefore, by the hypothesis, they should advance more towards a state of putrefaction by being boiled a few minutes, than by being kept a few days.

Upon a review of the whole of the experiments on this subject, I am inclined to think, that the loss of fixed air is rather a circumstance attending the putrefaction of bodies, than the cause of the putrefaction itself; and this I believe the more readily, when I consider that the greatest quantities of fixed air are given, by the author of nature, to bodies that are not putrescible,

cible, as the calcareous earths, and a variety of other things that might be mentioned. Now, if the intention of the fixed air in other bodies, is to preserve them from putrefaction, to what purpose is it given to bodies that are not putrescible?

CHAP.

CHAP. X.

Of the effects of damaged and mouldy provisions on the human body.

THERE are a variety of circumstances in life, which often oblige numbers of men, and other animals, to live long upon damaged and mouldy provisions, besides others commonly reckoned of an unwholesome nature. In order therefore to ascertain the effects of such provisions, we must consult the histories of those circumstances which have reduced mankind to the necessity of using them, as experiments cannot here avail us: for, to endeavour an elucidation of these effects by experiments on men, though possible, would be unhuman; ~~to do it~~ upon the other animals, would be tedious and uncertain.

Of all other histories, those of long sieges, and tedious sea-voyages, seem the most likely to afford us information concerning this subject, as it
is

is chiefly in these circumstances, that people are often obliged, by necessity, to subsist upon such things as they would otherwise abhor. But, as histories of this kind are seldom wrote by physicians, and often too by people unacquainted with philosophy, we cannot help deploring, that the information to be gained from them is far less than might otherwise be expected, and not at all adequate to what would be necessary toward clearing up this important point: for, in long sieges, we seldom meet with any other relation, than that the provisions were damaged; that, in consequence of this, a general mortality prevailed; while the more leading circumstances are usually left untouched, *viz.* what these provisions originally were, what changes they had suffered, and what proofs there were that, from these changes, the diseases themselves had arisen. In sea-voyages, when the crews of ships are attacked with the scurvy, the stale and salt provisions are immediately blamed: but it has appeared,

appeared, from what has been said concerning this distemper by the learned Dr. Lind, that it has often raged as severely among sailors plentifully supplied with fresh and wholesome provisions, as among those who fed on salted and mouldy ones *.

During the long and tedious siege of Jerusalem by Vespasian, though the besieged were reduced to the utmost extremity, though they used the most unnatural food, and though much of their natural food must, no doubt, have been damaged, yet we have no account of a plague, or any other putrid disease, appearing in the city †.

Among the great number of sieges related by Diodorus Siculus, I do not find that he mentions any city having been thereby affected with a plague, or putrid distemper; though he takes notice of a plague sometimes attacking the armies of the besiegers, as

* Lind on the Scurvy.

† Josephus' Wars of the Jews.

happened

happened to the Athenians, when besieging Syracuse, in the Decelian war *.

At the famous and memorable siege of Rhodes, when that city was defended so obstinately by the knights of Malta, against the numerous army of Solyman, there is no mention of any sickness among the besieged, though a mortality among the besiegers is frequently taken notice of †.

The allied army, under the command of the duke of Marlborough, in the war during the reign of Queen Anne, besieged and took several cities; but, in all these sieges, I find no account of any plague, or putrid disease, among the besieged; though it is reasonable to presume, that they were not always provided with fresh provisions ‡.

Among the Hottentots, at the Cape of Good Hope, where cleanliness and decency are hardly known, the most rancid and putrid provisions are fre-

* Diodorus Siculus' Historical Library.

† History of the Knights of Malta.

‡ Life of the Duke of Marlborough.

quently made use of; and yet they are said to be as healthful a people as are any where to be found in the world *.

The Eskimaux Indians, who live in the neighbourhood of Hudson's Bay, when other provisions fail, often live upon the skins which they had provided for trade, after having singed the fur off them. These, as they are not tanned, when either macerated in water, or in the stomach, must prove extremely rancid; but, even in this state, they have no ill effect upon the inhabitants, whom we would, perhaps, call wretched, because their sterile soil, and ignorance of culture, expose them to the necessity of using such food †.

The inhabitants about Joar in Africa live chiefly upon fish, which they lay up as winter-provision in their dwelling-houses; and these, being not thoroughly dried, generally stink so abominably, that an European can

* Roggewein's Voyage to the Cape.

† Ellis' Voyage for the Discovery of a North-west Passage.

hardly

hardly enter the house where they are. Another part of their food is ostrich eggs, which they relish most when so far hatched, that the chick is three or four inches in length*. There is, perhaps, no animal substance more offensively disagreeable, when become putrid, than eggs; and yet this inclination of the Africans seems probable enough, when we consider that several of the historians who have wrote concerning the inhabitants of the Northern nations, have taken notice, that those of them who live much upon the eggs of the sea-fowls which frequent their coast, likewise prefer such as are half-rotten to such as are perfectly fresh.

In some parts of the Highlands of Scotland, where the farmers rent large tracts of uncultivated mountains, a sheep that happens to die, either of disease or stress of weather, often lies till it is intirely rotten before it be discovered by the shep-

* More's Voyage to the inland Parts of Africa.

herd ; but, if he happens to find it before it be in its last stage of putrefaction, he carries it home on his shoulders, skins it, and delivers it to the landlady, who makes it into broth, and these broth, and the flesh, are used by the family, though both generally smell so much, that a person of any delicacy cannot enter the house where they are ; nay, sometimes, when the flesh of the sheep is judged rather too putrid for immediate use, they cut the body into four quarters, lay these in a rivulet, and fasten them with cords or stones for a day or two, then take them out and dress them as before : and yet, though this food is frequently used by them during the winter and spring seasons, they are a more healthful people than the inhabitants of better cultivated countries, who live in a more decent and cleanly manner.

After all these instances, one would almost be tempted to think that there is no difference in aliment, and that the stomach is endowed with a power
 6 of

of extracting good and wholesome chyle from every kind of it, in every state in which it can exist. But, on the other hand, when we consider the number of diseases which have from time to time been ascribed to damaged and unwholesome diet, though many of them, perhaps, took their rise from other causes, yet we cannot help believing that some of them arose from this only; and therefore we must give up the opinion, that every kind of food is equally nourishing and salubrious: and indeed, a tenet of this kind, were it upon no other account than that of decency and cleanliness, ought to be discouraged by every person of the smallest delicacy.

The foregoing account of the unwholesome food that is used, without producing any mischief, might easily be contrasted with numerous instances of the diseases and maladies it has brought upon mankind; but, as these are to be met with almost every where, I shall not take the trouble of transcribing them. We all know that

custom, with regard to the human body, becomes a kind of second nature; it is to custom therefore alone, that I can attribute the innocence of some of the kinds of food above-mentioned; nor can it be doubted, that they would be productive of a variety of diseases, were they to be adopted by people who have been brought up in a more clean and elegant manner.

For these reasons, though I would condemn damaged grain, and flesh already begun to putrify; yet I am far from thinking that salted meat ought to be condemned along with them, or that it is the cause of that putrid distemper called the scurvy, so often attributed to it; for we know that sea-salt is a strong antiseptic; and I am almost inclined to think, that, were it not for the salt provisions, the sailor who is attacked with a slow putrid scurvy, would fall a victim to the more rapid progress of a putrid malignant fever. To this it may be objected, that the scurvy is always cured by fresh provisions; but, let it be considered,

sidered, that these must be of the vegetable kind; for I know of no instance where fresh animal provisions alone ever had this happy effect.

Upon the whole, though I think necessity is the only reason that can be urged for the making use of damaged and putrid provisions; yet I will not take upon me to determine, whether they are the immediate cause of what we call putrid diseases; but if, either through a want of alimentary particles, or through a stubbornness and intractability in these particles to be assimilated to an animal nature, the body, by using them, be defrauded of its proper nourishment, or nourished in an improper manner; in all these cases, it will become a more easy prey both to putrid and other diseases; and, on this account, the choice of proper food is a very material article in the preservation of health.

C H A P. XI.

Of earthquakes, and other extraordinary causes, which have been said to be productive of putrid diseases.

IT seems to be an indisputable fact, that putrid distempers, anciently denominated by the appellation of *plague* or *pestilence*, have frequently followed soon after earthquakes. Earthquakes, therefore, in the times of superstition and ignorance, have, by many authors, been reckoned the cause of those diseases.

Trebellius, among several other causes, mentions earthquakes as one of those which produced a plague that infested the whole Roman empire in the year 263 *; and Eusebius says, that it reached even to Alexandria in Egypt, but attributes it to another cause †.

* Trebellius,

† Eusebius,

Kircher relates, that, in the year 746, a plague arose in Calabria and Sicilia, from an earthquake and an immoderate heat of the air; that the infection reached as far as Constantinople, which it almost depopulated: and he mentions another in Italy, which, he says, arose from an earthquake and eclipse of the sun*.

In the 16th century, an earthquake happened at Constantinople, by which the sea retreated suddenly about 2000 paces from the shore, and left a great number of fish upon dry land, which, soon after becoming putrid, are said to have brought a plague upon the city and country†. Another dreadful plague is also said to have immediately followed an earthquake that happened in Austria.

I could give many more instances, both from ancient and modern authors, not only of plagues having succeeded earthquakes, but of their having also been reckoned the natural conse-

* Kircher *Chronologia Pestium*.

† Marc, Anton. Plenc. *Tractat. de Terræ Mot.*

quences of these alarming phenomena ; but this would be foreign to my purpose : I shall therefore proceed to consider how far it seems probable that an earthquake may be concerned in the production of a pestilence.

If we consider an earthquake merely as a subterraneous power, which shakes nations, overturns mountains and cities, either by explosive fires, or confined air recovering its elastic force, we cannot see how it can be productive of a putrid contagion. The mere force by which the earth is shaken, can have no effect of this kind. Should its minerals and various other strata be laid bare, we know no mineral, nor other stratum, which has at any time emitted effluvia productive of a plague, though it may otherwise be detrimental to and destructive of health. Should marshes and stagnating lakes be shook and turned up from their lowest beds, it has already appeared, from experiments, that they are antiseptic, and would rather correct than promote putrefaction. Should
fulphureous

fulphureous or nitrous vapours, or flames, be vomited from the gaping earth, they would probably act as correctors of contagion : And should mephitic air be exploded, it must either kill instantaneously, or be dispersed in the atmosphere, and lose its destructive power. If an earthquake therefore have any power to produce a plague, it must act as a remote cause, and we must look for the proximate one in the effects produced by it.

When a violent shock of an earthquake happens in cities and towns crowded with inhabitants, as has frequently been the case, it is then productive of the most dreadful slaughter and devastation ; not only destroying great quantities of land animals of all sorts, but sometimes also throwing out, and leaving upon the shore, numbers of the inhabitants of the deep. These putrefying together with the other animals, and that part of the human species which, in times of such general calamity, often lie unburied, may certainly, in a warm climate, where

where there is little circulation of air, produce a local contagion, which may afterward be carried from one place to another by the infected, and at length become a general evil.

But this contagion, though it may perhaps of itself be strong and virulent enough to destroy mankind without any assistance, has generally several auxiliaries which help to facilitate its operations; these are, terror, idleness, and sometimes famine: for one shock of an earthquake being frequently a forerunner of more, after one has been felt, a terror is struck into the human mind, which hardly any one can conceive but he who dreads the stability of the ground upon which he treads, or that the house which he has reared for the convenience of his life, shall tumble down upon him and be his tomb. How terror of mind may facilitate the attack of contagion upon the body, I shall not endeavour to explain here, as I hope it will appear abundantly plain to every philosophical reader.

Partly

Partly from this terror, and partly from the loss of labouring utensils, and general confusion of property, which happens after an earthquake, almost all kind of labour is laid aside. A little attention to what happens every day (around us, will teach us that persons who have been long accustomed to labour, become a prey to many distempers, and are always among the first who fall victims to epidemical ones, when they suddenly desist from it. But this is not all; labouring people seldom have power, and not often inclination, to provide any thing for contingencies; hence a public calamity always attacks them unprovided, and, which adds to their misfortune, famine now lends her assistance to the other causes I have already mentioned; by all which, the body is soon debilitated, and becomes unable to resist a contagion, which formerly it would easily have conquered.

When we consider all these circumstances as the natural consequences of an earthquake, we need not be surprized

prized that a plague, and various other distempers, should follow it. We may therefore easily account for the fact so frequently mentioned by authors, without having recourse to any thing supernatural, or supposing that the earthquake itself is fraught with the seeds of contagion, which it disseminates abroad for the destruction of mankind.

Besides the causes of putrid diseases, which have been already examined in the preceding part of this work, there are a variety of others, which arose either from the prevailing philosophy of the times, from superstition, or from priestcraft; but they are most of them so whimsical, that they hardly deserve any serious attention: We shall just mention a few of them.

It has already been observed, that mankind, in every age of the world, have had a strong propensity to discover the cause of any thing that appeared strange and wonderful: stimulated by this propensity, they have generally fixed a cause for every phænomenon

non that fell within their observation, though unhappily they have often stumbled upon a very uncouth and unphilosophical one. Of this kind is that of Kircher, who, at a loss to discover the real cause of a pestilence, fixed it upon the venom emitted from serpents; not considering that serpents do not throw out their venom but either in their own defence, or to destroy their prey. But, even allowing that they did throw it out at other times, the quantity would always be too small to contaminate the atmosphere.

This, perhaps, was only the foible of a single person; but by much the most common of these whimsical opinions arose from judicial astrology, without the study of which, no man could formerly be a philosopher, and far less a physician. As no disease then, from a pestilential fever to a pimple on the nose, could attack a person, but by the malign influence of a planet or planets; so, far less could any disease be cured, without removing

removing this malign influence by the superior power of other planets that were more auspicious in their nature. This, however risible it may seem to us at present, about two centuries ago was reckoned so serious an affair, that he who had the greatest knowledge of the stars had likewise the greatest knowledge of the human body : and if a physician, or even an empyric, succeeded in curing a patient, where others had failed before him, his success was attributed to his superior skill in astrology ; which skill, at that time, was artfully employed to persuade mankind, that the stars were the cause of every public and private calamity that happened.

The doctrine of dæmons possessing a delegated power of contaminating the air, and producing pestilential diseases, was likewise often successfully inculcated upon the rest of mankind by the cunning priest ; and so far was this ridiculous opinion of dæmons interfering with the affairs of the air
and

and of mankind carried, that an author of reputation seriously relates a story of a woman, who met a dæmon while she was travelling, *qui morbum pudendi dedit ei in concubitu, de quo postea moritur mulier*. But I have already said enough concerning such whimsical notions, and shall now proceed to give my own opinion relative to the causes of putrid diseases, and their mode of acting on the animal body.

P

C H A P.

C H A P. XII.

*Of particular states of the atmosphere,
whether productive of putrid disorders.*

OF all those things which tend to support animal life, air is the most constantly and indispensibly necessary. It is a fluid, not only always in contact with the whole surface of our bodies, and absorbed by the cuticular pores, but also continually entering into our lungs, from thence passing into our blood, and constituting an ingredient in every part of our composition.—Our food, drink, and whatever else we make use of, are only necessary for us at some particular times ; but the air is necessary for us every moment. It becomes, therefore, natural for us to look upon a pure air as one of the greatest sources of health, and an impure one as one of the greatest sources of diseases ; and to attribute

to its various changes, from hot to cold, from moist to dry, and to the various particles which are continually floating in it, many of those disorders whose origin we cannot otherwise account for.

Formerly, when the plague visited Europe more frequently than it has done of late, when the minds of men were more clouded with superstition, and less acquainted with philosophy, that distemper was always reckoned the immediate effect of Divine vengeance. When philosophy began to emerge out of that Gothic darkness in which it had long been sunk, and natural causes consequently to be inquired into, the atmosphere was one of the first that attracted our attention; and whenever putrid diseases became epidemic, and no visible cause could be assigned, they were said to derive their origin from certain occult qualities of this atmosphere. These occult, or, as they were sometimes called, malignant qualities, were often attempted to be rec-

tified by large fires of aromatic woods, resins, gums, &c.; but these having never done any good, it became at last doubtful with some, whether such malignant qualities really existed; and as they could not detect them, they positively insisted that no state of the atmosphere had any influence on the diseases of the human body.—Others, willing to adhere to the old doctrine, still insisted on the malignancy of the atmosphere being the cause of every disease for which no other cause could be assigned. But as this atmosphere is so fleeting and volatile a body, and on these accounts cannot be properly examined, the dispute, in a great measure, remains as yet undetermined.

Some few attempts have indeed from time to time been made, in order to discover the qualities of the atmosphere, by exposing pieces of meat and other putrescible substances at different heights; but these trials have hardly ever led to any useful inference; and for this plain reason,
because

because there was no standard to compare them by; and without a standard, experiments of this kind can prove nothing; for, suppose a piece of meat to have putrified when exposed at any given height in an epidemical season, in the space of 12 or 14 hours, when the weather was of such a degree of heat and moisture, unless we could be certain that it would not putrify in so short a time in the same height, when the weather was of the same degree of heat and moisture, and no epidemical sickness existing, our experiment would prove nothing. For want of such a standard therefore, the only method of forming any idea of the consequences of experiments of this nature, would be to try the same putrescible substances, in the same altitudes, in healthful seasons, when the atmosphere, with regard to heat and moisture, exactly corresponded with its heat and moisture in such seasons as epidemic and contagious diseases, supposed to arise from the atmosphere, were prevalent.

This, I believe, was never attempted, owing, perhaps, to the difficulties that occur in the execution of it; for it is rare that our variable climate continues the same any length of time together, or is found the same any two different days, at different or at the same times of the year.

I have made two or three attempts of this nature of late, with a view to compare the qualities of the air at one time, with its qualities at another; but, from the whole, I can hardly say I have been able to draw any other inference, than that I have always found that pieces of meat which were pretty large, putrified more or less quickly, according to the different degrees of the heat of the air; pieces that were small, putrified in proportion, to the degrees of its heat and moisture; but none of those trials were ever made in states of the air, supposed to be epidemic or contagious.

Under the head of putrid effluvia, I have already delivered my sentiments
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concerning the possibility of the air being contaminated in our northern climates ; and though I have there given it as my opinion, that it never can be so contaminated as to infect people with putrid diseases, without the assistance of some particular contagion ; yet I will not pretend to say that there may not be some states of it, which may predispose the body both to these, and to a number of other epidemics. I am the more disposed to be of this opinion when I consider, that those gentlemen who have paid a more than ordinary attention to epidemics, have found them of such various natures, even when the climate and weather seemed to be exactly the same, that, not finding any other cause to deduce them from, they concluded, that they depended upon something in the atmosphere, which we have not yet, and perhaps never will have the smallest knowledge of *. But though we may

*Varii sunt nempe annorum constitutiones, quæ neque calori, neque frigori, non succo humido, ortum suum debent.

may not have it in our power to discover all, or even any of the hidden qualities of the atmosphere; yet, as far as its sensible qualities and mutations fall under our observation, we may endeavour to investigate them, and the effects arising from them,

We have already found in the experiments made upon heat and moisture, that they are both necessary to produce putrefaction; and, agreeable to this, it has always been observed, that a warm humid atmosphere has been followed with putrid epidemics; as plagues, so called in the southern regions, and malignant fevers, intermittents, and dysenteries in the northern, of which I could give many instances from authors, but shall content myself with only mentioning, that this is the pestilential state of air described by Hippocrates*, by Mer-

debent. Sed ab occultu potius, et inexplicabili quadam alteratione in ipsa terræ visceribus pendent, unde aer ejusmodi effluviis contaminatur. Sydenham de Morb. Epidem.

* Hippocrat. Epidem. lib. iii.

curialis,

curialis †, in the plague which happened in his time at Padua; and that at Smyrna, the plague, which is yearly carried there by ships, constantly ceases about the 24th of June, by the dry and clear weather they always have at that time, the unwholsome damps being then dissipated*.

As a continuation of warmth and humidity in the atmosphere has generally been observed to precede epidemics of the putrid kind, so in a low damp country, the same kind of diseases are generally endemic. These two facts being compared together, and found agreeable to the foregoing experiments, afford a strong proof of heat and moisture being favourable to putrid diseases, though I am inclined to believe that they cannot produce them without the assistance of some other causes; all therefore that they appear to do, is to predispose the body for the reception of a putrid infection,

† Mercurialis prælect. de pestilent.

* Mead on the Plague,

which,

which, when applied, will produce a putrid disease in such a state of the atmosphere; whereas in a dry warm, or in a dry cold state of it, it would perhaps not have been able to have produced any morbid affection at all; and this opinion seems confirmed by the history of plagues, which, in all countries, have generally been observed to abate something of their severity in warm dry weather, and generally almost intirely to cease in cold, dry, and frosty weather.

Let us now consider by what means a moist foggy atmosphere is productive of these effects.

In the first place, it may produce them by diminishing, or almost entirely stopping the perspiration, by which a superfluous load of noxious humours being confined in the body, it will become more liable either to fall into a spontaneous disease, or to receive the infection of a putrid one. Secondly, by weakening and relaxing the spring of the animal system,
and

and thereby diminishing its power of resisting any morbid contagion that may be applied to it. And thirdly, perhaps by introducing into the system a superfluous quantity of aqueous moisture, which has been found by some preceding experiments to accelerate the putrefaction of animal substances.

The learned Dr. Mead, in accounting for the origin of the plague, agrees with what I have here delivered, and says, that it is generated in Æthiopia during the rainy seasons; and, as he supposes it likewise to take its rise often in Cairo*, he imagines the cause of it to be the putrid effluvia arising from the canals in the city, as he could not derive it from the same cause as in Æthiopia, there being hardly ever any rain at Cairo†: But the doctor has here been in a mistake concerning the origin of the plague

* Mead on the Plague.

† Hasselquist's Travels.

in that city ; for we are informed by Prosper Alpinus, that it is very rarely, if ever, generated there, but commonly imported by trading vessels in the same manner as it is into Europe* ; but were it really a native of Cairo, from what I have already observed with regard to putrid effluvia, I am of opinion that it could not be generated there by moisture.

Next to the warm humid state of the atmosphere, the cold humid state of it seems the most favourable to the putrefaction of the living animal. It is to be observed, that this last state is not so favourable to the putrefaction of the dead animal, because heat, as well as moisture, is necessary ; but a living animal, having an innate principle of heat, which is almost always sufficient for this process, and which seems to suffer little or no alteration from the heat of the surrounding medium, is therefore in a state fit for

* Prosper Alpin. de Medicin. Egyptior.

becoming putrid, independent of any auxiliary heat. It becomes therefore a difficult question to determine, whether a warm humid atmosphere, or a cold humid one, is the most favourable to the growth and increase of putrid diseases in living animals.

Dr. Lind seems to be of opinion that heat and moisture in the atmosphere are most favourable to the rise and progress of acute putrid diseases, as pestilential and malignant fevers of all kinds; and that cold and moisture are most favourable to slow chronic ones, as the scurvy, &c. This doctrine he has illustrated with great propriety, from several instances of sailors having been attacked with the scurvy in cold damp channel cruises; and from the same disease being endemic among some of the northern nations, particularly such as live in damp unwholesome houses, and have scarcely any green vegetables during three-fourths of the year*.

* Lind on the Scurvy.

But,

But, as the natural heat of living animals seems to be but little, if at all, affected by the heat of the surrounding medium in which they live, it would from thence appear, that they should be equally subject to putrid diseases in a moist warm atmosphere, as in a moist cold one; and the history of most pestilential disorders seems to confirm this opinion.

It has been observed, that some substances, and particularly wood, is more liable to rot and decay, when exposed to many successions of drought and moisture, than when kept regularly dry or wet. This observation, however, as far as I can discover from the history of epidemics, is not transferable to animal substances; for I do not find that sudden† transitions from wet to dry, and *vice versa*, have been reckoned so hurtful to the health, as wet foggy

† Raulin des Maladies occasionées par les promptes et fréquentes Variations de l'Air.

weather

weather long continued: Nor do I recollect, among all the diseases mentioned by Raulin, as arising from sudden changes of the air, that he has mentioned putrid ones, though it would seem that such sudden changes may in some measure occasion them, by stopping the perspiration, bringing on a plethora, and afterward an inflammatory fever; which inflammatory fever may, and often does, end in a putrid one, particularly if it be improperly treated.

From the whole of the observations that have ever been made upon the atmosphere of this country, we have no great reason to dread its being the proximate cause of any putrid distemper; but as distempers of any kind are so apt to change their nature, and to be transformed into others quite different from what they were originally; and as the atmosphere of this country has frequently been the cause of diseases of divers kinds, when these have changed into putrid

trid ones, the air has often been said to be the cause of them, when it is probable they depended more upon improper management, or a variety of other circumstances.

CHAP.

C H A P. XIII.

*An attempt to explain how putrefaction
acts upon the living animal.*

FROM what has been said in the preceding chapters, as well as from daily observation, it appears evident, that all animal, and the greatest part of vegetable substances have a natural tendency towards a state of putrefaction, were they not perpetually restrained from that state, by the agency of some powerful cause or causes; so that, when we consider them with this tendency imposed upon them by the author of nature, we have more reason to wonder that they do not always run into a putrid state, than that they should only sometimes do so.

As in this enquiry we have found that heat and moisture, when applied to putrescible substances, universally dispose to putrefaction; and as the human species have always been

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found

found to possess a degree of heat and moisture highly favourable to putrefaction, let us first endeavour to investigate the reasons why that species, though it has the two most powerful causes of this evil interwoven with its existence, suffers so seldom by it ; and perhaps this investigation may throw some light upon the reasons why it sometimes does suffer by it.

In this investigation, the first, and perhaps the most powerful cause that presents itself to us, is motion. Before we attempt to explain the cause why motion is so strong and universal an antiseptic, let us first establish the fact*. This; I think, cannot be very difficult, for almost the whole of the works of Nature point it out to us. When we look around, and view the fabric of this material world, we hardly find any

* *Quamdiu æquabili motu per vasa circumducuntur humores, nulla nascitur in corpore putredo, omne illud, quod inciperet disponi ad putredinem, solitis corporis viis eliminatur. Vans-Wieten Commentar.*

thing at rest. In the deepest recesses of the earth, the different strata are perpetually concreting into new combinations, and forming gems, metals, spars, &c. On its surface it is constantly producing vegetable, animal, and other substances; from all which it appears, that its ultimate particles are in continual motion, independent of its annual and diurnal revolutions, by which the whole of it is moved.

The sea also, that huge body of water, which surrounds this earth, has the motion of a perpetual flux and reflux impressed upon it. The rivers are continually running from their sources towards the sea, and are by that motion preserved fresh; as it is only in some stagnating lakes and marshes that water is ever found putrid, according to the common phrase; though, in reality, neither water nor earth, considered as pure elementary substances, are putrescible; and it is only by being mixed with, and par-

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taking

taking of the smell of other putrid matter, that they have been reckoned putrid.

But, besides the earth and water, the air also which surrounds us is in perpetual motion, and constantly agitated by a variety of different causes. Though we do not know all the different particles which exist in the atmosphere, we are, I think, pretty certain that air itself is a non-putrescible substance; though at the same time we must also take notice, that there are always a great many putrescible bodies floating about in it, and these bodies, when they are accumulated together in large quantities, constitute what we call a putrid or unwholesome atmosphere.

All the various fluids contained in the animal system are in perpetual motion, and most of them, together with the greatest part of the solids, are concerned in those motions called vital or involuntary, which go on in a constant and uniform manner, whether the animal be sleeping or waking.

But, besides this, the whole body of every living animal is subject to the power of the mind, and by her directions frequently performing motions of the voluntary kind, though these do not seem to oppose putrefaction so much as the involuntary; for, when the vital or circulatory motion of the blood is stopped, in a leg for instance, and all the parts of that leg perfectly at rest among themselves, it soon begins to mortify, though the power of moving it by the direction of the mind may remain.

But not only the various parts of animals, but even those of vegetables also, are in perpetual motion among themselves. The circulation of their sap, is a fact that has been long established; nor, when we consider their daily growth, the formation of bark, flowers, and fruits, can it well be doubted that their solid parts have a species of motion also.

These few observations, out of a great many more that might be added, are, I presume, sufficient to

establish the certainty of all kinds of matter being in a perpetual motion ; and, whoever has attended diligently to nature, must, I flatter myself, have been convinced, that it is one of the principal agents by which bodies are preserved from a state of putrefcency ; and the reason of its acting in this manner will likewise appear from a little attention to the phenomena that attend putrefying bodies.

A great variety of repeated observations have taught us, that it is an invariable law of nature, that a fermentative motion must necessarily precede a state of putrefaction, in all dead animal and vegetable bodies. Now, in order to allow this fermentative motion to take place, it is necessary that the parts of the body upon which it is to operate, be at rest among themselves ; for, if they are urged by any *vis a tergo*, in the manner of living animal fluids or vegetable juices, they will be much less susceptible of motion from any other cause,

cause, which, before it can operate upon them, must destroy this force or power by which they are already impelled forward. Daily observation confirms the truth of this; for running water cannot be rendered putrid by adding a putrid ferment to it; nor will the limbs of animals, or branches of vegetables, become putrid, while they are properly supplied with juices from the parent stock.

A second cause, why mankind, though so susceptible of putrefaction, are so seldom attacked with it, appears to be the antiseptic nature of the food which they daily make use of. Though the human stomach has such an amazing power of converting almost all kinds of aliment into proper nourishment, that some have concluded it possessed of a power of destroying the specific difference of every thing taken into it*; yet experience teaches us, that the nourishment extracted from some kinds of food, has

* Arbuthnot on Aliment.

a much stronger antiseptic power than that extracted from others; according to which, it has been observed, that since sugar and the ascescent vegetables became a part of the diet of the people in this country, putrid diseases have been much less frequent than formerly, when our forefathers lived upon grain and flesh, without a proper mixture of acids to correct their tendency towards putrefaction.

There may be, and undoubtedly are, a number of other subordinate causes, which co-operate with these two already mentioned, in retarding the tendency of animals towards a putrid state. But as their power is, I presume, much more limited, I shall pass over them in silence, and only at present take notice of the effects of want of motion, and of antiseptic food, which, I am persuaded, will be found of themselves adequate to the production of putrefaction, without the existence of putrid miasmata, or any other cause whatever existing out of the body.

Neither

Neither the whole, nor any part of a living animal, is ever attacked with putrefaction, unless previously disposed thereto by the existence of some cause, though that cause may be not discoverable. On the other hand, the dead animal is always attacked with, and runs spontaneously into it, unless prevented by some evident cause. But the living and the dead animal are exactly of the same materials, and exactly of the same construction. That the living animal does not putrefy as readily as the dead one, must therefore be owing to something which it enjoys while living, and is deprived of when dead. We do not know in what life consists, nor the powers of which it is possessed; let us, therefore, instead of indulging ourselves in ideal conjectures concerning things which we do not understand, consider the difference between the living and the dead animal, as far as this difference falls within the compass of our knowledge, and try whether this will lead us to discover why

why the one seldom putrefies, and the other constantly does so.

Here the first and most obvious difference that strikes us, is motion; for, independent of the loco-motive faculty of a living animal, its various parts are all in constant motion among themselves. Now, as we have already seen above, that a state of rest is favourable to putrefaction, and a state of motion unfavourable to it*, we may, I think, from thence reasonably conclude, that motion is one of the causes of the preservation of the living animal, and the want of motion is one of the causes of the destruction or putrefaction of the dead one.

This conclusion admits of the strongest proofs, both from what happens to animals themselves, and from their analogy to vegetables. I shall begin with the former,

* The author here only means relative rest and motion; for it will appear afterward, that absolute rest, or immobility of the parts of any body, preserve it in the strongest manner; and that, on the other hand, violent motion in a living animal will destroy it, by inducing putrefaction.

Any part or member of an animal may easily be made to putrefy, by depriving it of the circulatory motion of its blood and other juices. If a ligature be tied around a leg or an arm, so tight as to stop the circulation, a mortification soon follows; when the influent blood is denied admission into any member from an internal obstruction, the same thing happens, if the obstruction be long enough continued; and in both these cases, the most obvious cause which we can discover, is the loss of motion.

Nor is it from animals only that proofs in support of this doctrine may be drawn: It will admit of further proof from the analogy of vegetables; for the same thing that happens to the member of an animal when tied with a tight ligature, happens also in the same circumstances to the branch of a vegetable: And indeed, all the succulent putrescible vegetables seem to depend more immediately upon the circulation of the sap for their pre-

preservation, as this circulation is no sooner cut off from a branch than that branch begins to putrefy, and resolve into its elementary principles, provided that its moisture be not too suddenly exhaled, so as to prevent this resolution from taking place, which a heat not very great will do even to the largest vegetables, and to large animals also, as is mentioned by the learned Dr. Shaw, who, in his journey from Egypt to the Holy Land, saw in the desert the bodies of some dead camels, which had belonged to a former caravan, exsiccated by the heat of the climate, so as to prevent that putrefaction which naturally consumes dead animals.

But, though the vital motion of animals and vegetables, when properly regulated, appears evidently to have a power of preventing putrefaction; yet this motion seems only to possess such a power, in consequence of its hindering the fermentative motion from taking place. Now this fermentative motion is absolutely necessary

cessary towards producing putrefaction in the dead animal, as will appear from considering that a state of absolute rest or fixedness of the *ultima corpuscula* of matter, is still a more powerful preservative than vital motion itself; for a piece of flesh, or of any other matter, whose parts are altogether fixed among themselves by frost, may be preserved perhaps for ever from putrefaction; and this, by the way, is one of the many facts that teach us how little we know of nature, and of the various and even contrary methods sometimes taken by her great Author to accomplish the same ends.

The next cause I mentioned, by which the living animal was preserved from putrefaction, was the constant supply of fresh and antiseptic materials, taken by way of food; let us therefore endeavour to investigate the manner in which food produces these effects.

I hope it will not be thought necessary here, that I should explain the
theory

theory of digestion, unless it be allowed that, by digestion, the aliment undergoes such a change in the stomach, that an antiseptic may be entirely divested of that quality, and acquire some other in its stead. But, were this the case, to what purpose do we prescribe antiseptics in putrid diseases? or, how should the internal use of the bark cure a partial or general tendency to mortification? Let us then take it for granted, that an antiseptic still continues to be an antiseptic, even after it has undergone the process of digestion; and, if so, the diet of most people will afford us strong reasons to believe, that they are in some measure preserved from putrefaction by its influence.

But it may be objected here, that, according to some of the best theories of digestion, the food itself undergoes a species of putrefaction before it is converted into proper nourishment. At first sight this objection seems to have some considerable weight; but, when we reflect, that
not-

notwithstanding this putrefaction, if it can be called such, the digestive powers extract a wholesome chyle from food fit for nourishing the body, it begins to be of less consequence; and it is of less still, when we further consider, that the product of a putrefactive mixture becomes an antiseptic, as has abundantly appeared from several of the foregoing experiments.

It may further be objected, that the food of many nations, as well as that of all carnivorous animals, is such as, having a strong tendency towards putrefaction itself, must necessarily augment that same tendency in the animals which live upon it. What has just been observed above concerning the product of a putrid ferment, will much diminish the force of this objection; and further, we must consider, that almost all carnivorous animals, whether of the human or any other species, are under a perpetual necessity of using exercise, in order to obtain their food, and this necessarily

necessarily preserving the vital motions from becoming languid and sluggish, may possibly preserve the body, by counterbalancing that tendency which would otherwise arise from the nature of the food made use of.

On a former occasion, I have proved by experiment, that sal nitre, a strong antiseptic, retains its nature and properties after it has gone through the circulation, and been excreted by urine*. May we not from this instance conclude, that many, if not all the other antiseptics, continue to be such after they have gone through the same process? And, if this be allowed, it affords an easy solution of the reason why antiseptic food preserves the animal that uses it from putrefaction. But, supposing the food of animals to have nothing antiseptic in its nature, yet the continual approximation of new and fresh parts to the body, in consequence of the use of it, must be a powerful preservative against putrefaction; agreeable to which, it has

* Vide Experimental Essays,

been

been found that people who have died of famine, have been in a highly putrid state before their death*.

An animal that is denied meat and drink, dies in a few days; but this death, I imagine, does not happen solely from inanition, but from a putrescency of the humours; as a proof of this, any watery liquor given to it, will keep it much longer from starving. Now, we can hardly suppose that the water does this by affording nourishment, as it is destitute of those mucilaginous particles which at present seem to be reckoned the nutritive part of our food; it must therefore do it by diluting the fluids, and consequently keeping them from this putrescent state. And this seems confirmed by experience; for people have lived upwards of twenty days upon nothing but water†; and the stories

* The blood of those that die of famine becomes highly acrimonious, which begets fever, phrenzy, and such a degree of putrefaction, as is highly destructive of the vital principle. Huxham on the Ulcerous Sore Throat.

† Philosophical Transact.

of much longer abstinence, where water has been allowed, are not altogether incredible.

Hitherto I have insisted upon motion and antiseptic food being preferatives against the putrescency of the animal body; the motion which I have always had in view, is that called vital, or involuntary, which must be carefully distinguished from that called fermentative; the former tending to the preservation, and the latter to the destruction of the animal. But though vital motion, when rightly regulated, tends to the preservation of the animal, when it runs to either extreme, it acts in a contrary manner. If it is too slow, it allows the fermentative motion to take place, and bring on putrefaction; if it is too rapid, it destroys the animal by bringing on this putrefaction itself*.

It

* *Longe autem citius in putredinem abeunt humores animalium, si valide moveantur, si valido cursu, vel alio labore, quis corpus exercuerit. Quam olidus sudor, quam acris et foetida redditur urina, dum febris acuta lactantem*

It is perhaps not easy to determine how a too rapid motion of the fluids should induce putrefaction; it is presumable, however, that it does so, by destroying their texture, and depriving them of their cohesive principle or bond of union, by the possession of which they are kept in a sound state, and by the loss of which they run into a morbid one. But, however this change is brought on, it certainly does happen frequently in consequence of rapid febrile motion, which has many a time changed the humours into a putrescent state in the space of a few days†.

What has been observed above concerning the effects of motion and rest, when applied to animal bodies, will, I flatter myself, enable us to assign a

lactantemprehendit mulierem? Nisi plurimum potest, intra paucas horas lac fiet tenue, subflavescent, falsum, odoris suburinosis. Van-Swieten. Commentar.

† *Nimia agitatio longe adhuc celerius putredinem inducit. Acutissima febris intra 24 horas sic potest corrumpere omnia, ut urina foetida, sordes alvinæ cada-verosæ penitus, halitus oris putridissimus, interna omnia jam corrupta testentur. Van-Swieten. Commentar.*

reason why these bodies are sometimes attacked with putrid diseases, without having recourse to συνάφεια μiasμῶα, or any of the other causes which attack the animal *ab extra*, as they point out to us, that animal matter has a constant tendency to, and is possessed of all the necessary requisites for running into a putrid state.

In order to illustrate this doctrine, let us take a view of the most simple methods by which a putrefaction is induced, and from thence proceed to the more complex.

The most simple method then of producing putrefaction is what I have already mentioned, viz. by tying a ligature upon a member, so tight as to cut off all communication with the vital parts, by which means it soon becomes putrid, without the assistance of any contagion, or internal disease of the body. This species of putrefaction seems to arise solely from the loss of motion.

The

The next method by which we see partial or universal putrefactions produced, is, when they happen in consequence of an inflammation. The manner in which an inflammation itself is produced has been much disputed, nor shall I at present enter into a discussion of that point; for my purpose, it is sufficient that in an inflammation there be a stricture on the parts, or an extravasation of humour, by either of which the regular influx of the blood is hindered: And that one or both of these always happen, I think, will hardly be denied; if the former, it will act in the same manner as a ligature, and by depriving the humours of motion, make them become putrid; if the latter, the extravasated humour, being without the reach of the moving powers, must stagnate; and, if this stagnation be long enough continued, it must putrefy also. Here, likewise, the putrefying cause is loss of motion*.

Such

* Though I have said above, that putrid diseases may arise without the assistance of contagion, I do not thereby

Such putrid diseases as attack the whole system, may arise either from putrid miasmata being received into it, from the perspiration being stopped, from the circulation being rendered too slow, or too rapid. When the first is the case, the learned Dr. Huxham supposes that the miasmata are swallowed along with the saliva, where they act as a ferment on the contents of the stomach. Now, it is a well known fact, that fermentation is, of all things, the most favourable to putrefaction. These putrid miasmata then being received into, and mixed with the contents of the stomach, already fermenting, will change the product of that ferment, from a mild nutritive liquor, to an acrid and putrid one; in consequence of this, the chyle and blood must receive some of this product into their composition, which, acting as a stimulus upon the heart and arteries, will excite them to more strong and mean to preclude its agency, as they may no doubt also be caused by it.

frequent

frequent contractions; hence a too rapid motion of the blood will ensue, its texture will be destroyed, and having lost that, it will be easily assimilated to the nature of these putrid particles that were mixed with it. This is one of the putrid diseases caused by too much motion.

In fevers of this kind, when nature is tolerably strong, and the extremities of the excretory vessels at the same time shut up, she endeavours to propel these putrid particles towards the surface, where, meeting with an absolute resistance, they can proceed no farther; and thus being lodged between the cutis and cuticula, form what are called petechiæ, livid spots, &c. The matter of these petechiæ being thus extravasated, generally become more and more putrid, as they are fomented with heat and left in a state of rest; when nature, finding that in this state they would produce further mischief by contaminating the parts around them, makes another effort

to get clear of them, which is done by absorbing them again into the system, with a view to throw them out some other way; in which, if she succeeds, the patient recovers; if not, death puts an end to the conflict.

That putrid contagion makes its first attack in the manner I have now related, seems highly probable, from a diligent attention to nature; as one of the first symptoms, after having been infected, is generally a nausea, and inclination to vomit, which, if timely encouraged, and a gentle diaphoresis brought on afterward, seldom fails to avert the impending danger; many instances of which have been related to me, by gentlemen who have been abroad in the warm climates, and several I have seen in this country.

It may be asked here, why this putrid contagion should produce so much mischief when taken into the stomach, when it has already appeared from several facts, that putrid aliment

ment has been frequently made use of for a long time together, without having any bad effect? To answer this, we must consider that this kind of contagion, taken in along with the air of an infected place, is already become highly subtilized and virulent, before it is fit to float in the air; whereas the other is generally but in the first, or at farthest in the second stage of putrefaction, and may therefore be thrown out of the stomach, or even out of the blood, if it should have got in there, before it has acquired a sufficient degree of virulence to enable it to act as the former.

The second method I mentioned, in which putrid diseases attack the human species, is in consequence of the perspiration being stopped. And I am inclined to think that they more frequently happen in this way than by any putrid ferment, or other cause *ab extra*. When they do happen in this way, then the perspirable matter,

ter, by being retained, becomes highly acrimonious: Nature, finding it endowed with this quality, raises a fever to throw it out; but, if the skin is impervious, instead of being thrown out, it is absorbed into the mass of blood, in consequence of this fever, where, by its stimulus, it hurries on the circulation in such a manner, that a decomposition and putrescency of all the humours ensue.

From one of these two causes last mentioned, I imagine the greatest part of putrid fevers, of whatever denomination, arise. But as there is, besides fevers, another putrid disease, viz. the scurvy, which advances in a much slower manner, and is hardly attended with any febrile symptoms, let us examine it also.

Though the scurvy has been known to attack mankind in a variety of different circumstances of life, these attacks have always been irregular and inconstant. But there are two causes, which, when they

meet together, and are long enough continued, seldom fail to produce it in a regular and constant manner. These are cold and moisture, which may therefore be denominated the real causes of scurvy; and that they act in concert by diminishing the force of the moving powers, I shall endeavour to prove.

The momentum of the circulating fluids seems to depend intirely upon the strength and elasticity of the solids. But it is a fact established by experience, that nothing tends more to destroy the firmness and elasticity of the solids than too much moisture: every one must have felt languor and debility after having been long exposed to a moist foggy atmosphere; and the debilitating effects of large quantities of watery liquors are well known. Moisture then, acting long upon the body, must weaken the moving powers, and thereby the circulating fluids, or parts moved, must creep on more slowly, and thence a fermenta-
tive

tative motion be more apt to arise in them; for it seems to be a law of nature, that all animal and vegetable matter must either be subjected to a vital or fermentative motion; and, as soon as the former fails, the latter necessarily begins, unless the *ultima corpuscula* of the matter be fixed by some powerful cause, as frost, exsiccation, &c.

But, besides this debility of the moving powers, which arises from too much moisture, cold likewise co-operates with it in producing the same effect; but then we are only to understand this of the severer degrees of cold, when joined with moisture, and continued for some considerable time; for a moderate degree of cold, on the contrary, braces and strengthens the solids; but, when it is severe, when it is long continued, almost every one knows its effect to be numbness, loss of feeling, and inability of the muscles to act in their usual manner; by which, and by the relaxing power of moisture, the circulation

lation must become slow, when, by that law of nature which I mentioned above, a fermentative motion must begin, and increase, as the vital motion decreases, till at last a putrid diathesis is generated, which will take place first in the extremities of the exhalent arteries, where the circulation is slowest; and hence the livid spots and blotches which so often appear in scurvies.

If I have succeeded in describing the manner in which putrefaction attacks animal bodies, the conclusion that will naturally follow is, that the living animal can only become putrid by a defect or excess of the vital motions; and therefore, whatever causes this defect, or excess, will bring on a putrid state of the humours. This being the case, we need not look out with so much solicitude to discover the causes of putrid diseases in effluvia, animalcula, &c. as we will find that they can easily arise without any thing being externally applied to the body;

body; for animal matter, being of all others the most disposed to putrefy, will spontaneously run into that state, whenever the causes that perpetually hinder this disposition are taken away or suspended.

After this attempt to examine the causes that have generally been assigned for putrid diseases, and to explain their manner of operating on the body, it may be expected that I should say something concerning the method of curing them. Should I do this, I could hardly add any thing new upon the subject; as it has already been treated in a learned and judicious manner by Sir John Pringle, Dr. Huxham, and several others, whose names will be a perpetual honour to the healing art.



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